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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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AN INVESTIGATION OF ESP BETWEEN TEACHER AND PUPILS IN AMERICAN SCHOOLS

By J. G. Van Busschbach

ABSTRACT: A first report is given here of the repetition in American schools of the investigation Mr. Van Busschbach made of ESP between teacher and pupils in Dutch schools. Reports of the Dutch work have appeared in earlier issues of the Journal. The method was what is known as GESP. The teacher, concealed and located in the rear of the classroom, looked at a card selected at random from a set of five, while the pupil attempted to identify the card. Ten trials were made with each of three different sets of cards, one set having five arithmetical symbols; another, five colors; and the third, the names of five common objects. Thus 30 trials were given for each session. The test was designed for the fifth and sixth grades and the project called for about 20,000 trials as a total. Significantly positive results were obtained, first in Amsterdam and then in the schools of Utrecht, where some research assistants substituted for Mr. Van Busschbach. This independent confirmation justified the further extension of the investigation to the schools of Durham and Burlington, North Carolina. Only slight modifications were needed in the technique. Again the results were positively significant. In all three investigations—Amsterdam, Utrecht, and the United States—insignificant results were obtained from tests with higher grades. In all these locations, similar differences were shown in the scoring of pupils on the three different sets of target cards, the color targets being the most favored.

Mr. Van Busschbach came to the Parapsychology Laboratory of Duke University on a grant from the Parapsychology Foundation and a Fulbright travel grant. He was on leave from his position as Inspector of Schools in the City of Amsterdam in the Netherlands.—Ed.

The Background: Tests in Dutch Schools

In my professional work in the schools of Holland I have repeatedly encountered examples of what appeared to be an extrasensorial transfer of thought. These instances raised the question whether such

interpsychical transfer might not play an important part in the teacher-pupil relation. For the purpose of beginning an exploration into this possibility, I conducted a series of investigations on extrasensory perception in Dutch schools in 1951 and in the years that followed. In these studies I tested the success with which a classroom of pupils could acquire knowledge by purely extrasensory means concerning the order of cards which were looked at, one-by-one, by their teacher, who occupied a screened position at the rear of the room. In each class tested, each pupil made 10 trials with each of three different sets of target cards, making 30 trials altogether. One set of the target cards was made up of five arithmetical symbols (+, −, O, ×, and :). Another set was made up of colors (red, blue, green, orange, and yellow); and the third consisted of words denoting five familiar objects: *stoel* (chair), *bord* (blackboard), *raam* (window), *deur* (door), *krijt* (chalk), all one-syllable words with different vowels. An experimental assistant observed the pupils and kept the records in order.[1]

The test was designed for the age level of ten to twelve years, corresponding to the fifth and sixth grades in Dutch schools. In later research, the same procedure was used with pupils of higher grades. Some tests were done also in which an adult stranger or one of the pupils was substituted for the teacher as sender.

The results gave a clear indication of the presence of extrasensory perception in the fifth- and sixth-grade children when the teacher served as sender. The first series, made in Amsterdam with the fifth and sixth grades, contained 20,190 trials and gave 174 more successes than the 20 per cent expected from pure chance. This was a statistically significant result ($CR = 2.79$; $P = .005$). With the collaboration of school officials and of the Parapsychology Institute of the University of Utrecht, a repetition of the experiments was conducted in the city of Utrecht. Children of the fifth and sixth grades in that city made a total of 26,880 trials, with 179 hits more than "chance," which, like the Amsterdam series, is a significant result ($CR = 2.73$; $P = .006$). Together these two Dutch experiments give a total result that is highly significant ($CR = 4.07$; $P = .00005$).

[1] A more detailed description of the procedure is given in the first *Journal of Parapsychology* report (see Reference 1).

The tests with the pupils of higher age range gave only chance results, as did the tests in which someone other than the regular teacher served as the sender or agent.

The American Series

In 1956, I received an invitation from the Parapsychology Laboratory at Duke University to conduct a similar investigation in American schools. The Parapsychology Foundation of New York gave me a grant to aid in that undertaking, and a Fulbright award was received from the United States Government. These generous provisions and a three-months' leave granted by the Dutch Minister of Education made it possible for me to spend the greater part of March, April, and May, 1956, in the United States in the effort to repeat the work I had done in Holland.

In addition to the projected undertaking in testing school children in American schools, my stay at the Parapsychology Laboratory provided me with opportunities for analysis and discussion of results, both European and American, that have brought out still further valuable information beyond the lines of analysis previously followed. It would be a large undertaking to assemble everything under the heading of a single paper and these further analyses will therefore be reported later. It seems better to confine the scope of the present report to the main results of the American investigation, with only a few very general comparisons with the Dutch work already reported.

It is perhaps of importance to say first a word about the attitude of the school authorities, teachers, and pupils with whom I have worked in the United States. In Holland I was not only in my own country, but, as Inspector of Schools, I was officially at home as well, and in a position of some authority. I could not have been more generously aided in the American schools if I had been one of the school officials myself. On the part of everyone, the cooperation was very generous and was accompanied by a positive interest in the experiment. This was a very important condition in itself.

The Parapsychology Laboratory placed at my service Miss Margaret Anderson and Miss Rhea White who were especially interested in the experiment, and later, for a shorter time, Mrs. Winnifred Nielsen and Mrs. Betty Shaw. Superintendent L. S. Weaver

of the Durham City Schools provided me with the assistance of Mrs. Rosa Tillitt, Supervisor of Elementary Schools. These assistants had entire charge of the conduct of the tests in the classrooms, but they received exact instructions (translated from the Dutch work) on how to administer the tests. Thus there was uniformity in the explanations given to all classes.

Although the main objective was the continuation in America of the original Dutch tests with the fifth and sixth grades, it was considered advisable, especially in the last part of the research, to make a few changes, if only to add a certain desirable novelty to the program. As one modification, there were to be no other senders or agents than the teachers themselves. Shortness of time was a consideration in this decision. The word targets and the arithmetical symbols were altered slightly. The words used in the American tests were *door, desk, wall, page,* and *seat.* The arithmetical symbols were +, −, O, ×, and ÷. In Holland small rewards were given to the winning pupils in each run of 10 trials; in the American tests it was decided to substitute a movie ticket. There were, of course, certain changes that were inherent in the situation. Most of the teachers in America in the fifth and sixth grades are women; in Holland they are nearly all men.

The testing began in the city of Durham, N. C., and was conducted on 23 sections or classes of fifth- and sixth-grade pupils. It was continued in the neighboring city of Burlington, in which 17 classes were tested. This involved a total of 1207 pupils in both cities and a total of 40 teachers, one man and 39 women who, of course, served as senders. Altogether this yielded a total of 36,160 trials.[2] As may be seen in Table 1, they gave a positive deviation of 226 hits with a critical ratio of 2.70 calculated by the Greville method, which is appropriate for tests in which groups of subjects call the same order of targets.[3] This critical ratio is almost identically the same as those obtained in Amsterdam and Utrecht, although the block of trials in America is somewhat larger. The percentages

[2] This figure takes account of the fact that the subjects failed to mark a call in 50 instances.

[3] Because of the amount of work involved, the Greville method was not applied to all the data. The instances in which it has not been used are either results which do not go beyond the range of suggestive significance (Tables 2 & 3) or are so highly significant (the combination of the Dutch and American work in Table 4) that the corrections would not change any conclusions or interpretations.

Table 1

TOTAL RESULTS FROM FIFTH AND SIXTH GRADES, CITY SCHOOLS OF DURHAM AND BURLINGTON (COMPARED ON BASIS OF SEX OF SUBJECTS, TYPE OF TARGET MATERIAL, AND SUBJECTS' GRADE IN SCHOOL)

	Trials	Hits	Percentage of Success	Deviation	CR	P
Boys	19,142	3,953	20.65	+124.6	2.25*	.024
Girls	17,018	3,505	20.60	+101.4	1.94*	.05
Symbols	12,057	2,476	20.54	+ 64.6	1.33	.18
Colors	12,062	2,583	21.41	+170.6	3.52	.0004
Words	12,041	2,399	19.92	− 9.2	−0.19	.84
Fifth Grade	15,966	3,269	20.47	+ 75.8	1.31	.2
Sixth Grade	20,194	4,189	20.74	+150.2	2.49	.013
Total Results	36,160	7,458	20.63	+226	2.70	.007

*The CR's for the breakdown between boys and girls was calculated by the formula which assumes a binomial distribution of scores, Dev. $/\sqrt{npq}$. All the other CR's in this table were obtained by the Greville method for evaluating multiple-calling data. The CR of the total results obtained by the Greville method is 90.9 per cent as large as the binomial CR for the same number of trials and deviation. This agrees closely with the Amsterdam series, for which the Greville CR was 91.2 per cent as large as the binomial CR.

of success were 20.86 for Amsterdam, 20.66 for Utrecht, and 20.63 for Durham and Burlington combined, where 20 per cent is the level of mean chance expectation (see Table 4).

Although the sixth grade did somewhat better than the fifth in the American city-school series, it is not a significant difference, and the boys and girls were about evenly divided in their performance. The outstanding feature in the results was the fact that colors stood out well above symbols in rate of success. Colors alone gave independently significant results with a CR of 3.52 by the Greville method. The symbols gave a positive deviation, but the CR of 1.33 is not significant. The words were almost exactly at the level of mean chance expectation. Because of the design of the experiment it is not possible to say whether colors were so outstanding because they always came second in the order in which the tests were given or whether they would have stood out in any case. Because I wanted to do the tests in America in essentially the same way I had done them in Holland, I did not at this point introduce the desirable change of varying the order in which the three sets of cards would be used, although this was done later. But color had stood out as the best target in the Dutch work as a whole and it was interesting to see this feature confirmed also in the American work.

To extend the American comparison with the Dutch work done in higher grades, a series of 20,160 trials were made with seventh- and eighth-grade pupils in the city schools of Burlington, N. C. These tests actually contributed a substantial negative deviation, as can be seen in Table 2, a trend that was divided between both grades. The only difference in the test procedure for the seventh and eighth grades in America from that followed in Holland was that it was considered advisable to use the practice of giving small rewards. That had not been done in Holland. In view of the chance character of the results, however, there is nothing of importance to report in consequence of the changes. These results are, in general, comparable to the Dutch results for higher grades.

Table 2

TOTAL RESULTS FROM SEVENTH AND EIGHTH GRADES IN THE CITY SCHOOLS OF BURLINGTON

	Trials	Hits	Dev.	CR
Seventh Grade.....	13,650	2705	−25	−0.53
Eighth Grade.....	6,510	1279	−23	−0.71
Total Results.....	20,160	3984	−48	−0.85

With this confirmation of the main general features of the work that had been done in Holland, both in its evidence of ESP in the fifth- and sixth-grade tests and its failure to find such evidence in the seventh and eighth grades, it seemed desirable to branch out into a new area as far as possible in the time remaining. No work in the rural schools of Holland had been completed at the time these tests were made, but such a project was going on during my absence in America. I undertook, therefore, to carry out the next part of the investigation on the fifth- and sixth-grade classes of the Durham County school system. It was at this stage that the additional assistance was needed, and Mrs. Nielsen and Mrs. Shaw joined forces with me in the undertaking. Miss Anderson and Miss White, on completion of their program with the seventh- and eighth-grade pupils in Burlington, came in on the county program as well. Some further changes in procedure were introduced. The order in which the three types of target cards were presented was systematically

varied, and the word targets were changed from the names of five common objects to the names of five animals (dog, cat, lion, bear, and pig).

The county school investigation calls for a special analysis, particularly in view of an unforeseen situation which was encountered. Although nothing was lacking in the way of interest and cooperation on the part of administration and teachers, it was found that, because of differences in the geographic and administrative setup, the timing and provision for preparation of the teachers for the advent of the experimental assistants was not adequate in the county school program. In some instances when the assistants arrived to give the test, the teachers were taken almost by surprise. Sometimes they did not understand the nature of the tests sufficiently and were not psychologically prepared for the role they were to play.

At the end of the first 6,714 trials,[4] a negative deviation of −45.8 had accumulated and it was decided to try a period of teacher preparation for the rest of the county program. From that point on, then, each teacher was visited by the experimental assistants a couple of days before the tests took place, and the program was explained. The appointment for the tests was cleared, and every effort was made to see that the tests were properly anticipated. Thereafter, in the 14,818 trials[5] that followed, a deviation of +78.4 was obtained. This gives a percentage of hits approximately the same as that obtained by the fifth- and sixth-graders in the city schools in Durham and Burlington. While the total deviation is not itself independently significant, the difference between the two stages (before the teacher preparation was instituted and afterwards) gives a CR of 2.06, which is not far short of being acceptably significant.

Perhaps the most constructive suggestion from this experience is the added emphasis on the importance of adequate preparation. Presumably, the teacher is the person most concerned, at least most obviously so.

There are some other effects that are quite apparent although they should be taken only as suggestions at this stage (see Table 3). The rotation of the order in which the different sets of targets were presented makes it possible to compare the effects of run posi-

[4] This figure is corrected to take account of 36 calls that were not marked.
[5] This figure is corrected to take account of 2 calls that were not marked.

tion on scoring. The largest deviation was on Run 1 in Section B of the table; in fact, this has a significant CR (2.56) when taken by itself. It is noteworthy that when the tests were going negative, as in Section A of the table, Run 1 gave the largest negative deviation (CR = −1.39). It is, of course, well known from previous work that the first run in a series is the one most likely to achieve outstanding scores. The symbols stood out more prominently in the positive scoring and the colors in the negative. Neither one was significantly outstanding.

Table 3

RESULTS FROM FIFTH AND SIXTH GRADES IN RURAL NORTH CAROLINA SCHOOLS (COMPARED ON THE BASIS OF SEX OF SUBJECTS, RUN POSITION, AND TYPE OF TARGET MATERIAL)

	A			B			C		
	Tests Previous to Time Teachers Were Given Preparation by Assistants			Tests Given After Teachers Were Prepared by Assistants			Total		
	Trials	Dev.	Percentage of Success	Trials	Dev.	Percentage of Success	Trials	Dev.	Percentage of Success
Boys	3,534	+10.2	20.29	7,228	+41.4	20.57	10,762	+51.6	20.48
Girls	3,180	−56	18.24	7,590	+37	20.49	10,770	−19	19.82
Run 1	2,242	−26.4	18.82	4,940	+72	21.46	7,182	+45.6	20.63
Run 2	2,236	−10.2	19.54	4,939	+ 8.2	20.17	7,175	− 2	19.97
Run 3	2,236	− 9.2	19.59	4,939	− 1.8	19.96	7,175	−11	19.85
Symbols	2,236	− 6.2	19.72	4,939	+39.2	20.79	7,175	+33	20.46
Colors	2,236	−42.2	18.11	4,940	+28	20.57	7,176	−14.2	19.80
Words	2,242	+ 2.6	20.12	4,939	+11.2	20.23	7,181	+13.8	20.19
Total	6,714	−45.8	19.32	14,818	+78.4	20.53	21,532	+32.6	20.15

CR_d (binomial) = 2.06
P = .04

It is now in order to make a few general comparisons of the main results from the two countries. For this purpose it will be best to confine ourselves to the fifth- and sixth-grade tests in the city schools. These results are summarized in Table 4. It will be seen that the percentage of success in the totals declines from Amsterdam to Utrecht and from Utrecht to the U.S.A. While this

decline is not statistically significant, it suggests the possibility that some inspirational factor may have been reduced as the program was systematically extended. Such has been a general rule over the past history of psi investigations. There are subtle psychological variables always operating in the educator-pupil relation and it is well known that they are especially effective in experiments in extra-sensory perception. As the analyses of these data continue, there will be more to offer on differences which suggest the operation of interpersonal factors in the test situation.

Table 4

TOTAL RESULTS OF THE FIFTH AND SIXTH GRADES FOR CITY SCHOOLS IN HOLLAND AND NORTH CAROLINA

		Symbols	Colors	Words	Total
Tests in Amsterdam Schools	Trials	6,730	6,730	6,730	20,190
	Dev.	+ 72	+ 53	+ 49	+174
	Percentage of Success	21.07	20.79	20.73	20.86
	CR (Greville)	2.01	1.46	1.37	2.79
Tests in Utrecht Schools	Trials	9,070	9,070	8,740	26,880
	Dev.	+ 41	+141	− 3	+179
	Percentage of Success	20.45	21.55	19.96	20.66
	CR (Binomial)	1.07	3.70	.....	2.73
Tests in N. C. City Schools	Trials	12,057	12,062	12,041	36,160
	Dev.	+ 64.6	+170.6	− 9.2	+226
	Percentage of Success	20.54	21.41	19.92	20.63
	CR (Greville)	1.33	3.52	− 0.19	2.70
Total N. C. and Holland	Trials	27,857	27,862	27,511	83,230
	Dev.	+177.6	+364.6	+35.8	+579.0
	Percentage of Success	20.64	21.31	20.13	20.70
	CR (Binomial)	2.66	5.46	0.55	5.02
					P = .0000006

The similarity in the relative success of words, colors, and symbols in the two countries is a rather striking aspect of the results. The percentages are almost identical. The highest success was obtained on colors in both cases and the lowest on the words, the actual percentages being: for colors, 21.23 in Holland and 21.41 in the U.S.; for symbols, 20.72 in Holland and 20.54 in the U.S.; and for words 20.30 in Holland and 19.92 in the U.S.

It is interesting, then, to recall some of the differences in conditions: In Amsterdam the experimental assistant was always myself. In Utrecht the assistants were a colleague of mine and two graduate students from the University of Utrecht, one of them experienced in the classroom and the other not. In the American work the experimental assistant for the Durham City schools was Mrs. Rosa Tillitt, who, as Supervisor of Elementary Grades, was, of course, thoroughly experienced in the classroom. In the Burlington schools the work both for the fifth and sixth grades, as well as that for the seventh and eighth grades, was done by Miss Anderson and Miss White, at first working individually, one in each classroom, and at a later stage working together, the one devoting attention to the assistance of the teacher and the other supervising the children.

It seems important to note that a number of different experimental assistants have taken part in the three investigations and yet, as has been said, the scoring rate has been remarkably consistent for the three series. It seems safe to suggest, therefore, that the teacher-pupil relation in the fifth and sixth grades provides a ready-made relationship for the exericse and demonstration of ESP.

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ESP TESTS AT LONG AND SHORT DISTANCES

By KARLIS OSIS[1]

ABSTRACT: This is a report of an experiment in which a single subject called ESP target cards at both close and long distances in the same session. The close distance targets were directly in front of the subject, who called "down through" the shuffled decks. The long distance ones were 4,000 miles away. The total deviation on the close distance targets was highly significant, while the average score level on the long distance part of the experiment was at the chance level. However, there were certain effects which indicated the presence of ESP in the long distance data. For instance, there was a significant negative correlation between long distance and close distance scoring in the experimental session; that is, when the scores on close distance increased, the scores on long distance decreased, and vice versa. Such a correlation gives indication of the presence of ESP in both the close distance and long distance trials, though it was only at the close distance that the subject was able to hit the target to a significant extent. Additional evidence for ESP in the long distance trials was found, however, in certain other effects described.—Ed.

INTRODUCTION

MANY EXPERIMENTS have been reported in which ESP subjects were separated from their targets by long distances (**1-14**). The problems dealt with in the early experiments of this kind were necessarily limited in scope: Does ESP work at long distances, and if so, at how great a distance?

The card experiments of Rhine and Humphrey (**10**) and of Rhine and McMahan (**11**) with a subject in Yugoslavia, and Carington's work with drawings (**2**) showed some evidence that subjects can use ESP over distances of thousands of miles. The first objective of the present experiment was to verify these findings.

The following question also was raised some decades ago: Does distance, as such, influence ESP efficiency? Some results concerning that question have been published (**1, 3, 9, 12**). However, in none of these researches were all the variables which might have

[1] I wish to express my appreciation to Prof. Ernst Haeckel not only for his important role as subject but also for his participation in the planning of the experiments and in the checking of the results, as well as to my colleagues in the Parapsychology Laboratory for many helpful contributions to this research. Mr. Malcolm Turner gave valuable assistance on the statistical evaluation of the results. This work was supported in part by a grant from the Rockefeller Foundation and in part by the Parapsychology Foundation.

influenced the scores equalized. For example, the experimental procedures did not provide for the use of the same subjects by the same experimenters with the same agents and testing techniques at both long and close distances used in the same session. It would give a more effective comparison if the same subject were asked to do long distance (LD) and close distance (CD)[2] runs at the same session, ideally without knowing at which distance he was working in any one run. But even without this ideal situation, tests at both short and long distances in the same session might provide a basis for closer comparison between LD and CD scores. Such a comparison was one of the exploratory aims of the present experiment.

The comparison of LD and CD scores is complicated, of course, by the very likely possibility that the subject's attitudes towards CD and LD may be different. If the subject considers CD a boring condition and LD relatively more challenging, he may do better with LD. If the subject feels that distance is a real barrier to his ESP, he may score low on LD for that reason.

Some indications of whether preference factors were operating might be given in the results, such as by the sign of the deviation being different for the two conditions, or by a significant negative correlation between the scores on CD and LD. Therefore, a study of associations between scores on both tasks by means of the product moment correlation between LD and CD hits on the direct targets was planned from the outset. A significant correlation between LD and CD irrespective of the amount of total deviation would, of course, provide evidence of ESP effects in the data.

The distance experiments of Carington (2) and Marsh (6) showed a considerable dispersion of ESP hits under the LD conditions in the form of displacement. A comparison of dispersion under the LD and CD conditions was one of the exploratory aims of the present experiment. However, in the present report of the investigation, the chief emphasis is on the question of the occurrence of ESP at a long distance. Because of the difficulties of performing a distance test with adequate control of the psychological factors, the results do not go beyond the exploratory stage of considering the question of whether there is any measurable effect of distance, as

[2] The term "close distance" will be used instead of "short distance" to avoid the use of "SD" which might be mistaken for "standard deviation."

such, upon the ESP process; and no effort will be made in this paper to interpret the results in relation to this problem.

The subject in these experiments was Prof. Ernst Haeckel, formerly a professor of German languages in Budapest who resides in Garmish-Partenkirchen, Germany, a city in the middle of the German Alps. Prof. Haeckel has had a prolonged interest in parapsychology and has written two papers concerning the field. He has obtained significant results in self-testing experiments and he believed he could do as well in LD tests. This sort of attitude and interest seemed to be appropriate for such an extensive experiment.

Procedure

I served as experimenter throughout the entire experiment. There were three series: A, B, and C. The respective lengths of the series, insofar as LD was concerned, were 20, 20, and 60 standard runs of 25 trials each, using the ESP test cards. The general scheme of the experiment was planned in advance, but the later series incorporated changes based upon the accumulating experiences from the first and second series.

In all three series the LD runs were to be done first in each session, followed by CD runs. The reason for using this order was to give the more important LD part of the experiment the most favorable position in the session.

The subject, in Germany, and I in Durham, N. C., synchronized our watches by radio signals. The ESP target cards in the distance tests were exposed in my room in the Parapsychology Laboratory at specified times and in a certain manner which was different for each series. The subject made his calls for the LD targets simultaneously with their exposure by the experimenter.

Prof. Haeckel's Series A and B records were mailed to me before I sent him the target orders. In Series C the records were mailed from both points simultaneously and they crossed en route. We always held to the agreed dates for mailing.

After the subject completed his long distance calls, he made his CD runs in all three series "down through" ESP card packs which he had randomized by shuffling and a cut. He kept the cards screened from himself and he did his own recording and the first

checking. These records were rechecked in the Parapsychology Laboratory.

Series A

Series A consisted of 20 sessions with one LD run and one CD run in each. For the LD run, a closed deck of 25 ESP cards was laid out on my desk, face up, just before it was time for the experiment to begin. The deck was arranged in two parallel columns of 13 and 12 cards. The segments of five cards in the run were indicated by pulling each fifth card a little out of the column. The deck was chosen randomly from a number of decks, each prepared by means of random number tables, and was not used again. On two occasions, shuffled decks were used, but they were two different decks not previously used in the experiment.

I concentrated on each card, beginning at the top of the first column and working through the whole deck. I spent 50 seconds on the first card, paused 10 seconds, then concentrated on the next card an equal period, and so on. Concentration consisted merely of looking at each card while the entire 25 remained face-up before me.

Series B

Series B consisted of 10 sessions of two LD runs, or 50 trials per session, completed without interruption, and two CD runs. This uninterrupted double run of 50 successive trials was proposed by Prof. Haeckel and agreed to by me. I anticipated that there would be a definite decline effect within the session.

I used 100 ESP cards each enclosed in an opaque envelope. The envelopes were coded with some Latvian syllables representing the various ESP symbols. This was done in order to enable me to think in Latvian terms, which Prof. Haeckel would not consciously recognize, and not in terms of visual images or of English or German words, which he would recognize. The recording was done in the same Latvian manner. Thus it seemed it would be less likely for the subject to read my records or my memory instead of the actual cards.

I chose by random cut one card from the pack of 100 for exposure. I put it on the desk before me for 20 seconds without seeing the code, which lay face down. During the next 10 seconds, I

looked at the coded symbol and recorded it, put the card back in the pack by random cut, and took up the following card in the deck. The envelopes were then reshuffled and the experiment proceeded in this manner throughout the 50 trials. This randomizing procedure was adopted as possibly relevant to a question regarding the relation of ESP to distance which is not discussed in this paper.

I asked Prof. Haeckel to do two CD runs after the long distance runs in order to get an adequate comparison. However, he misunderstood me and did two CD runs before and two CD runs after the long distance ones. I evaluated both sets of CD runs but only the planned runs were taken for comparison.

Series C

The procedure was modified in Series C to make it still more likely that any ESP effect would be obtained only through clairvoyance at the time of the experiment. Series C consisted of 15 sessions in each of which four LD runs were followed by one CD run. For each session there were four "open" decks of ESP cards in unsealed, coded black envelopes, each envelope containing one card. The decks were prepared from random number tables by an associate in the Parapsychology Laboratory, and I did not know the card orders during the session. Each deck was grouped in five small subdecks containing five envelopes kept together by an elastic band. Each packet had a code number. A board, with 20 numbered squares (five across and four down) was by my side. The subdecks were laid out on it in such a manner that each square contained a subdeck. I also had a pack of 20 cards with the numbers 1-20 which I shuffled and kept face down. From this pack I chose one card by cutting, and picked up the subdeck which lay on the square of the corresponding number. I held that subdeck in my hand for 50 seconds; and in the next 10 seconds I recorded its code number, put it back, and chose another subdeck by cutting the 19 cards remaining in the numbered deck. There was a five-minute pause after the second run. The target records were decoded by the member of the Laboratory Staff who prepared the runs.

For the sake of simplicity other details of the procedure which are relevant only to the exploratory aspects of the research are not included in the present report. The author will be glad to furnish

additional information about the experiment to other research workers who are concerned with the question of the effect of distance upon ESP.

Table 1

General Results of All Three Series

Series	Close Distance					Long Distance				
	Trials	Av. Score	Dev.	CR	P	Trials	Av. Score	Dev.	CR	P
Direct Target										
A	500	....	+36	4.03	.00006	500	....	+18	2.01	.044
B	500	....	+ 7	....	....	500	....	− 2		
C	375	....	+19	2.45	.014	1500	....	+ 8		
Total	1375	6.13	+62	4.18	.00003	2500	5.24	+24		
Backward Displacement										
A*	408	....	+13.4	....	....	480	....	−10		
B	480	....	+ 7	....	....	480	....	+ 3		
C	360	....	+ 1	....	....	1440	....	+17		
Total	1248	5.21	+21.4	....	....	2400	4.90	+10		
Forward Displacement										
A*	408	....	+ 2.4	....	....	480	....	− 1		
B	480	....	+ 6	....	....	480	....	+12		
C	360	....	− 8	....	....	1440	....	− 7		
Total	1248	4.81	+ .4	....	....	2400	4.84	+ 4		

*Displacement could not be evaluated on first three runs of Series A because the target order was not reported.

Results

In accordance with the experimental plan, direct hits as well as +1 and −1 displacement were checked (see Table 1). The total deviation on direct targets was highly significant in the CD runs but insignificant in the LD runs. The difference between the two is associated with a CR of 2.54. Displacement deviations are insignificant and so are the differences in displacement scores between both conditions.

The direct (0) scores of the CD part of the experiment are independently significant for Series A and C. It should be remem-

bered that in Series B the subject misunderstood the instructions and made CD runs before and after the LD runs. Only the planned CD runs done after the LD have been used in the evaluations, as planned. However, the 20 CD runs done before the LD are highly significant (Dev. = +34; CR = 3.80) and the apparent decrease in scoring between these and the CD runs done after the LD runs (see Table 1) might be interpreted as a session decline effect.

Table 2

Correlation Between Daily (Session) Scores on Close Distance and Long Distance Targets

Series	Sessions (N)	Correlations (r)	P
A	20	−.70	.001
B	10	−.44	.1
C	15	−.30	.3
Mean correlation: $\bar{r}$ (39 d.f.)		−.54	.00038

The second analysis planned was the correlation between LD and CD in terms of the number of hits on the direct target in a session. Table 2 summarizes the results of the correlation studies from all three series, only that from Series A for the direct scores being independently significant. However, the correlation coefficients based on the direct scores from all three series are consistently negative in sign and the mean value of the correlation coefficients is −.54 (39 d.f.) which has a P value of .00038. Fisher's method was used for obtaining the mean of the correlations ($\bar{r}$).

Table 3

Distribution of Hits on Long Distance Direct (0) Targets in Terms of Successive Run Scores Within the Session in Series B and C

Series	Runs	Total Scores				Chi-Square Test			F Test		
		1st Run	2nd Run	3rd Run	4th Run	χ^2	d.f.	P	F	d.f.	P
B	20	63	35	..	..	9.95	1	.0016	11.48	1, ∞	.0007
C	60	79	86	75	68	2.78	3	.43	.96	3, ∞	.96

The distribution for run scores within the sessions was studied to test the prediction for Series B and C that a decline of scoring

would occur. In the LD trials of Series B a marked decline was found from the first to the second run in the session (see Table 3). In the LD trials of Series C a decline in the four runs was also found but it was very slight. The evaluations are given in Table 3 both in terms of a chi square test and by the method of analysis of variance which gives a more sensitive test of the effect in question

Table 4

ANALYSIS OF VARIANCE ON DIRECT TARGET IN LONG DISTANCE DATA

Source of Variation	Degrees of Freedom	Sum of Squares	Mean Squares	F*	P
Series B					
Between Sessions	9	342.7757	38.0561		
Between Runs Within Sessions	1	376.9724	376.9724	11.48	.0007
Sessions x Runs	9	483.1526	53.6836		
Totals	19	1202.9007			
Series C					
Between Sessions	14	581.9295	41.5664		
Between Runs Within Sessions	3	94.2697	31.4232	.96	.96
Sessions x Runs	42	1556.6501	37.0631		
Totals	59	2232.8493			

*The data were submitted to an arc sin transformation and therefore have a constant theoretical variance $V\phi = \frac{820.7}{25}$. In this case, $F = \frac{820.7}{25} = 32.828$. This is the error variance used for obtaining F values here. (See K. Mather, *Statistical Analysis in Biology*. New York: Interscience Publishers, Inc., 1947.)

(see Table 4). The chi square test of Series B is based on a two-by-two table; the one in Series C on a two-by-four table which took both hits and misses into account. By both methods, the decline in Series B is significant ($P = .001$ by the chi square method; $P = .0007$ by the analysis of variance) whereas the distribution in Series C is not. The difference between the declines in B and C is significant. It was tested by means of a t-test of regression coefficients b_1 and b_2 ($t = 3.15$; ∞ d.f.; $P = .002$).

We come now to an analysis which was first made in part of the data on an exploratory basis. This was a study of the vertical distribution of hits within the run. It was first carried out on the LD data of Series B, where it was discovered that the (-1) scores

gave an uneven distribution for the five segments of the run. The distribution yielded a chi square of 14.01 with 4 degrees of freedom ($P = .007$). This same analysis was then applied to the (-1) scores of Series A and subsequently to the (-1) scores of Series C to test the prediction that a similar decline would be found in those

Table 5

Distribution of Hits on Long Distance Backward Displacement Targets in Terms of the Five Segments of the Run

Status of Analysis	Series	Runs	Total Hits					χ^2	d.f.	P
			1st Segment	2nd Segment	3rd Segment	4th Segment	5th Segment			
Exploratory	B....	20	24	14	32	18	11	14.01	4	.007
Confirmatory	A....	20	22	10	28	18	8	15.72	4	.0035
	C....	60	66	48	80	53	58	13.85	4	.008
Total (A+C)	..	..	..	..	..	..	..	29.57	8	.00027

results as well. In each of these series the segment-to-segment variation in scoring was found to be independently significant. Table 5 summarizes these results. For the confirmatory analyses in Series A and C the total chi square was 29.57 (8 d.f.; $P = .00027$).

Beyond the mere fact of nonrandom distributions by segments in all three series, the distribution patterns were found to be strikingly similar (see Fig. 1). The association between the patterns was tested by means of Fisher's intraclass correlation (see Table 6) and is significant ($P = .007$).

Table 6

Intraclass Correlation of Backward Displacement Scores for the Five Segments of Long Distance Runs in Series A, B, and C

Source	d.f.	Sum of Squares	Mean Square	F	r_I	P
Between Segments.....	4	235.9728	58.9932	6.92	+.66	.007
Within Segments.....	10	85.2872	8.5287	....		
Totals.....	14	321.2600				

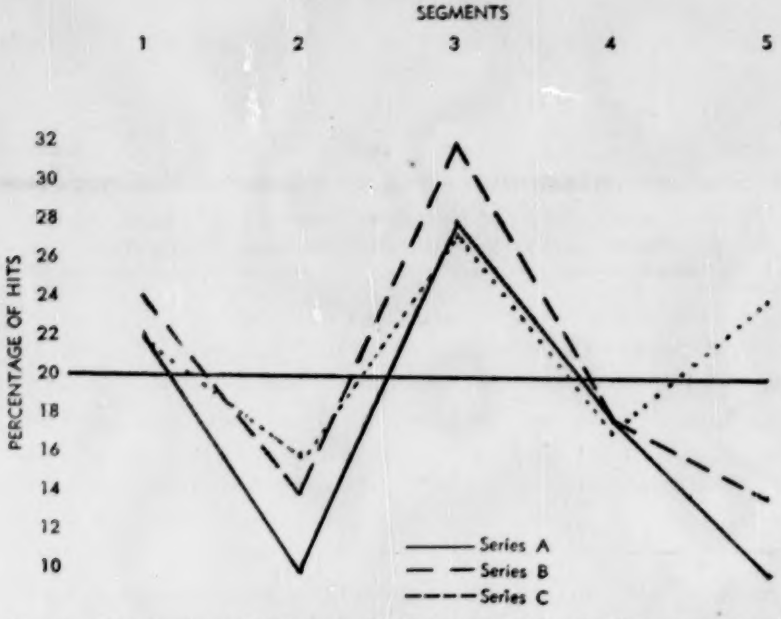

FIG. 1. Distribution of hits on (−1) displacement for the five segments of the run in the long distance data of Series A, B, and C.

Chi square analyses similar to those in Table 5 were also performed on +1 displacement and direct target scores in the LD runs without finding even suggestive effects.

DISCUSSION

Statistical Significance and Safeguards

A matter of over-all relevance is that of safeguards. In Series A and B I sent the records of the targets for each LD session to Prof. Haeckel only after I received his call sheets. In Series C the records were sent on appointed days by both participants. The postmark dates indicated that Prof. Haeckel always kept the appointed times. Therefore, in making his calls he could not have used other information than that obtained by ESP.

All the data were independently rechecked. The CD run records depend largely on the accuracy and honesty of Prof. Haeckel, for they were done completely under his control. However, the signifi-

cant correlation obtained would be unlikely if both the LD and CD trials did not reflect ESP effects.

The experiment was analyzed in many ways. Let us consider first the question: On which measures can the claim of nonchance effects be based, and how strong are these effects?

There is no significant deviation on the LD work but there is a significant deviation on the (0) target in CD. And there is a significant correlation between daily scores on LD and CD in Series A. In Series B and C the correlations are not independently significant, but the mean of correlation coefficients for all series is significant. The use of this statistical measure was anticipated in the experimental plan. One important bearing of this finding is that it offers an indirect validation of ESP in the high scores of the less rigorously controlled close distance trials. If those scores had been spurious for any reason, we would not expect any correlation between the CD and LD results.

One of the most interesting measures in the experiment is the distribution of hits according to run position in the session. However, since in Series A there was only one run in the session, the hit distribution in question does not apply to it. In Series B a run-to-run decline in the session was predicted. Therefore, the significance of these effects (Table 3) may be accepted at their face value ($P = .0007$). In Series C the decline was predicted again. However, the observed curve of hit distribution shows a modified pattern. The second run of the four-run session yielded the highest deviation. There was a slight decline between the first and second halves of the session. Neither the predicted measure nor the detailed analysis of the observed distribution gave suggestive departure from chance in Series C.

The third strong point in this experiment is the vertical hit distribution in (-1) displacement according to the five target segments in the run (see Fig. 1 and Table 5). This effect was first found in the analysis of Series B ($P = .007$). It was then verified in the data of Series A ($P = .0035$), and predicted for Series C ($P = .008$). This secondary effect was therefore both suggested and confirmed in the course of the experiment.

Displacement Increase in Distance ESP

This experiment does give certain answers to the question of whether there were greater displacement effects in Prof. Haeckel's long distance than in his close distance runs. In the CD runs there was a highly significant total deviation on the direct target (Table 1); however, there were no significant displacement effects either in the individual or the pooled CD totals. Apparently in these runs the subject was successful in scoring hits on the direct targets without dispersion into secondary effects. In the long distance runs there were no significant deviations on the direct, −1, or +1 targets. The presence of ESP was indicated in a more indirect manner by the significant mean correlation with CD scores, by the significant decline of run score within the session on the direct target in Series B, and by the significant and consistent variation of scoring in the segments of the run on the backward displacement in all three series. It is impossible to make an exact comparison in this instance from one target category to the other since the effects were detected by different kinds of measures.

We might ask further: Is the increase in displacement in LD tests caused by a psychological blocking effect over distance due to the subject's attitude, or is it caused by distance itself interfering with ESP? The present experiment does not give us anything definite on this question. The presence of shifting attitudes is indicated by the strong correlation between LD and CD. I tried to counterbalance the psychological blocking effect of distance by putting the CD runs at the end of the session, but there is no way to ascertain how effective this counterbalance really was.

Associations Between LD and CD Direct Scores Within a Series

The associations between scores in a dual task in itself is an interesting problem. G. Murphy and E. Taves (**7**), some 17 years ago, carried out a large scale experiment designed to investigate the problems of the multiple task in an ESP situation. They used four kinds of stimuli in each session (playing cards, ESP cards, crosses and circles, and circles and blanks), expecting significant covariation between scores on the various aspects. They obtained a low but statistically significant correlation ($r = +.11$) between scores on some of the tasks, despite the fact that the total scores closely ap-

proximated chance expectation. However, at least one of the authors at present considers the correlation explainable as an artifact (according to response to a recent inquiry).

Dr. R. H. Thouless (**15**) in one of his experiments used three procedures in the same session: a DT test, and two kinds of precognition tests. He obtained negative deviations on procedure A (clairvoyance) and positive deviations on procedures B and C (precognition). Scores on A and C were significant. In spite of the different signs of the deviations of the scores obtained by these different methods, there was a positive and significant intraclass correlation between them.

It is my own experience that significant correlations between scores on multiple tasks in ESP tests are not the rule. In the present experiment there was an unusually strong negative, rather than positive, correlation. This is the opposite of the finding in the experiments mentioned above. It is one more indication that the multiple task situation releases different types of consistent responses in different subjects.

Thouless has given this interpretation to negatively correlated scores with different signs of deviation: "I had expected a negative correlation between a task scoring positively and one scoring negatively. This follows from the consideration that the subject needs psi either to score significantly above chance or to score significantly below chance. If on some days my psi capacity was working well I should have expected, therefore, to score high on B, and to have had large negative deviations on A. While on the day when psi was working badly, I should have expected to have small deviations on both tests" (**15**, p. 271).

Examination of the data of Series A in this experiment, where the greatest correlation was apparent, shows that this interpretation does not apply to the scores of Prof. Haeckel. The negative correlation between daily scores on LD and CD targets indicates that when LD scores are good, CD scores are low; conversely, when LD scores are poor, CD scores are high. As far as I can see, it indicates frequent changes in the subject's attitude toward both tasks. Apparently the psychological blocking effect of distance is not a consistent factor, nor a particular characteristic of Prof. Haeckel's long distance ESP. Apparently the subject's interest and readiness for the LD

task often outweighed his interest and readiness for the CD task.

The frequently fluctuating attitudes of the subject which are indicated by the correlation coefficient seems not to be a good explanation for the persistent differences in scoring patterns between the LD and CD parts of the experiment. Extensive research with many subjects and more sophisticated methods where subjects' attitudes are taken into account will be required to reveal the full effect of distance upon ESP efficiency. At present, there is no empirical basis for any conclusion on this question.

Conclusions

A) This experiment gave some verification of the evidence for long distance ESP. The distance was approximately 4000 miles.

B) An increase in the dispersion of hits in the LD part of the experiment was found. Some clues were given for further research about factors underlying the observed dispersion.

C) There was a significant negative correlation between the LD and CD tasks. This was interpreted as being due to the subject's fluctuating attitude towards the tasks.

D) A reliable comparison of the subject's ESP efficiency in the LD and CD situations was not possible because of limitations inherent in the data.

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MENTAL STATES ASSOCIATED WITH SUCCESS IN PRECOGNITION

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ABSTRACT: This experiment in precognition was carried out with eight subjects, each of whom contributed 100 runs of 25 trials each, made in five-run sessions. The novel feature of the experiment is the attempt to obtain from each subject at each test session a self-rating which would provide an estimate of the subject's general feeling of well-being (referred to as "mood" in the article). The preliminary rating scale devised for the purpose stressed the subject's estimate of his own physical vitality, mental alertness, and contentment or emotional well-being. The total of 800 runs gave a quite significant positive deviation that was relatively consistent for both series of the total experiment. The majority of the sessions were done when the subjects were in their pleasant "moods" and this may be the reason for the large positive result. Significantly high ESP scores were produced when the subject gave high ("pleasant") ratings in all three divisions of the scale. When the ratings gave a divided reaction (that is, when a pleasant mood was indicated in only one or two of the three divisions of the scale), the ESP results tended to approximate chance. However, the subjects gave their highest scoring rate of all in a small section of data associated with consistently "unpleasant" ratings on all three parts of the scale. Consistency of response in all three branches of the scale thus seems more important to ESP than response in any one branch. The ESP results in the experiment support the case for precognition; with regard to the question of the relation of ESP and mood, they are suggestive, but somewhat inconclusive.—Ed.

INTRODUCTION

ONE OF THE BASIC AREAS of research in parapsychology is concerned with the question of favorable experimental conditions for the occurrence of psi. Although we cannot as yet get evidence of psi in every experiment, it is one of the intermediate steps to observe and register as far as possible the conditions under which it occurs. Such an approach could lead not only to improved experimental control but also to more understanding of the psi process.

As a large part of the immediate psychological environment through which psi must be mediated, the subject's "mood-of-the moment" enters in as one of the conditions of every test session. In this usage, the term obviously involves many factors. Some of

[1] The research reported here was carried out partly by means of a grant from the Parapsychology Foundation and partly with the aid of a Ralph Drake Perry Fellowship in Parapsychology.

these are emotional in origin, some even physiological. The resulting complex mental state will be referred to, for convenience, as a "mood," but the term is admittedly given broad coverage. It seems worth while to ask, then, whether there is any evidence of variation in ESP scoring levels in association with mood changes, and whether this variable can be brought under any degree of experimental control.

Investigators have long noticed that scores were likely to be better under optimal subjective conditions of well-being, happiness, and so on. Some of the general impressions on this point are included in Smith and Gibson's summary of conditions affecting ESP performance (**4**). Experimental reports of varying kinds also refer, incidentally, to the importance of the subject's mood.

However, little experimental work dealing directly with a quantitative study of this factor has been published.[2] Perhaps the most closely related to the question of mood and ESP is Humphrey's work with ESP drawings classified as "expansive" or "compressive" according to the Elkisch criteria. Humphrey found that the same subject's drawings might be rated as "expansive" at one session and "compressive" at another, and that in clairvoyance tests the "expansive" drawings gave more evidence of ESP than those which were rated as "compressive." (In GESP tests this relation was reversed.) Her interpretation was that the expansive-compressive rating "helps us to catch something of the subject's change in mood and also helps us to understand a little better the reasons for a subject's day-to-day fluctuation" (**2**).

Another experiment which is somewhat related is Woodruff and Dale's study of the relative importance of various subject and experimenter attitudes. In a 25-item questionnaire presented to all subjects, the authors included the question, "Did you feel in a good mood when you came for the experiment?" On most of the 25 items, the two experimenters obtained opposite scoring trends from the same subjects; that is, with Dale, affirmative anwers to the questions tended to be associated with positive ESP results, while with Woodruff as experimenter the reverse was true. Although the results did not reach significance, it may be of some interest to

[2] Mr. G. M. Fisk has recently carried out a study, not yet published, along these lines.

note that the mood question was one of the few not associated with this opposite scoring trend. With the subjects of this experiment, a "good" mood was associated with negative deviations and a "bad" mood with positive scores (**5**).

In the field of psychology also there have been comparatively few studies of mood; and most of these have been concerned with the behavioral and subjective correlates of mood as estimated at a single experimental session. As far as I have been able to ascertain, only two have considered the problem of changes in mood as related to changes in "behavior." Hersey studied emotional cycles in a group of workers over a period of about a year and related the cycles to changes in productivity (**1**). In panel studies of social change Kendall related mood changes to instability of response to the same questions presented twice (**3**). There was no mood test in general use; in both of these studies the authors worked out their own methods of mood measurement.

The purpose of the present research was to frame an experiment in which the relationship, if any, between moods and ESP test results could be explored. Such a study should provide for the measurement of mood at the time of each ESP test session, and it should include repeated sessions for each subject so that consistency of relationship between mood and ESP in the same person might be studied. Experimental conditions should allow as much freedom and spontaneity as possible for the subjects so that any effect on mood of matters of procedure could be held to a minimum. The primary emphasis should be on catching psi occurring under conditions that arose naturally in the everyday life of the subjects. It would be necessary, then, to make up a scale by which a subject could rate his moods, and from which data could be obtained concerning relationships between them and his ESP scores. Also of course, an effective test for ESP would have to be chosen or devised. Both of these phases of the project are described below.

Procedure

The ESP technique chosen was that of precognition. There were several reasons for this choice. The procedure would allow the ideal conditions of virtually complete spontaneity for the sub-

ject so that all possible advantage might be taken of mood changes. Also, the results of precognition tests have often shown negative deviations or displacement effects, or both, so that the question of what conditions may permit positive on-target deviation would be especially interesting if they were to occur.

In making up the rating scale for enabling the subject to estimate his mood at the time of the ESP test, the aim was to use descriptive terms that would be familiar to the subjects. Accordingly, it seemed best to use non-technical language in the scale and instructions. Likewise, in dealing with a problem so necessarily exploratory, a certain license is required in shaping up a scale to fit the conceived needs of the project. One may thus take some liberties with considerations of overall consistency and technical precision of language in this first stage of trying to catch preliminary signs of meaningful relationships. The scale in use in this experiment was therefore a groping effort, without benefit of expert training, experience, or counsel. It is reported because, to a limited extent, it appears to have served at least its introductory purpose.

A tentative scale was constructed which provided for ratings of what could popularly be called "physical," "emotional," and "mental" states as the individual might regard them. The "emotional" section was originally emphasized to the extent of consisting of three subdivisions, A, B, and C. At the midpoint of the experiment, however, A and B were found to be slightly less revealing than C, and for the remainder of the experiment (Series II), only C was used. The original scale thus had five subdivisions throughout the first half of the experiment, each with a choice range of 1 to 7, with a short description accompanying each rating. Some of these descriptive statements were made in a deliberately unreserved and emphatic way in order to discourage the subject from checking them except when he was feeling distinctly out of the ordinary, in which case one would suppose they would not sound so extreme.[8] The general aim was to arrange the ratings in each section in a roughly graded scale from "pleasant" to "unpleasant."

[8] The two male subjects found these extreme statements embarrassing and out of place. One used them occasionally even so; the other confined his ratings to the midrange.

The following is a reproduction of the rating scale given to each subject.

PHYSICAL VITALITY

At the moment, I feel:

1. Wonderful
2. Pretty good
3. Unaware of my body unless I think about it
4. Tired
5. Exhausted
6. Unpleasantly keyed up and nervous
7. Ill

EMOTIONAL VITALITY

(A) Today, I feel as if:

1. I could lick anything
2. Life is good
3. I can take care of things
4. I can manage if I have to
5. To heck with it
6. I wish things were different
7. No use at all in anything

(B) Right now, I feel:

1. Very happy
2. Optimistic
3. Nothing's bothering me
4. A little worried
5. Irritable or angry
6. Really afraid about something
7. Deeply lonely

(C) At this moment, my feeling toward people in my life is:

1. Loving without reservation or consideration for self
2. Wanting to give more than my share
3. Perfectly willing to do my part
4. I don't *really* mind
5. Tired of getting no appreciation or understanding
7. Sometimes I feel as if I don't even like them. I hate feeling this way!

MENTAL VITALITY

At the moment, my mind feels:

1. Exalted by prayer or other great emotional experience
2. Highly stimulated by creative insights
3. Alert and responsive to new ideas
4. Busy with everyday considerations
5. Distracted
6. Tired
7. Bored

Six members of the Laboratory Staff (four of whom, including myself, were actively engaged in research) participated as subjects. In addition, a Durham resident who visited the Laboratory and a correspondent who is a serious student of parapsychology were asked to take part. The experiment took place during the summer and fall of 1955.

Each of the eight subjects agreed to complete 100 runs of ESP symbols. Thus the total number of runs was set in advance at 800. Each subject's 100 runs was to be completed in two 50-run series, each series consisting of 10 five-run sessions. The division into two series was made for several reasons: to provide a psychological break in a long number of runs, and to see whether there would be similarity of scoring patterns between series in association with moods. Series I also served as a trial period during which the practical value of each of the five sections of the scale was assessed.

The subject recorded his calls on standard ESP record sheets which he filled out at a time and place of his own choosing. This procedure is possible in a precognition test since the target orders for a given session cannot be determined until after the calls for the session are complete. Thus the subject was free of the need to make test appointments. He also was given a copy of the mood scale, which he kept in his own possession so that it would be immediately available whenever he desired to have a session.

It seemed best to have the mood ratings made immediately following each session; the subject did not, of course, know his test scores until after the ratings were made. Reading over the statements on the scale in advance might have had an adverse effect on ESP.

At each session, then, the subject made five runs of precognition calls at any time or place and under any circumstances he chose; he then immediately rated his mood by selecting one descriptive statement from each section of the scale for each session. During Series I, the first half of the experiment, the subject rated himself on each of the five sections of the scale, and the experimenter made a comparison of the ESP scores associated with each of the five parts of the scale considered alone. As stated, parts A and B of the "emotional" section were dropped before Series II; part C was retained

for the remainder of the experiment along with those for "physical" and "mental" vitality.

The method of selecting targets for the sessions was especially important since the tests were of the precognitive type. The point of emphasis in such a case must be the determination of a target *after* the call has been made, in such a way as to eliminate the possibility that the target is in any slightest degree made to fit the call. In other words, it must be beyond question that the selection of the target is independent and random. On that account, the procedure, as set up in advance of the experiment, was as follows:

A separate set of targets was obtained for each individual session. To obtain this, a 10-face die was thrown from a cup 10 times to get a 10-digit number, which I recorded, digit by digit, on the record sheet to which it belonged. The number was then multiplied by its own reverse on an electric calculator. Pairs of digits from prespecified positions in the product were used to give a random entry point into the Kendall and Smith random number table. Because of the exploratory character of the experiment, I did not feel at the outset that involving a second person's time and attention to the many separate target entries was justified. At the end of Series I, however, a small refinement in target procedure was introduced; throughout Series II, the die-throwing and recording of the target computation number of each session were witnessed by a member of the Laboratory staff.

All data have been independently rechecked by another member of the Laboratory staff. Rechecking included not only the accuracy of the target orders and of the scoring, but also recalculation of the target numbers to insure that entries into the random number tables were accurate. The scoring errors that were found served to increase the deviation from that originally found, because there was a tendency for errors to occur in overlooking hits. In addition, all scores in association with mood ratings have been independently retabulated.

Results

In both series the precognition tests yielded significant results. Series I gave a deviation of +114 with a critical ratio of 2.85; Series II gave a deviation of +97 with a critical ratio of 2.42. The

total deviation, then, was +211, and the overall critical ratio, 3.73. The associated two-tailed P value of .00019 is corrected to .00057 because +1 and −1 displacement were also checked (see Table 1). These figures are, of course, highly significant and indicate the occurrence of ESP of the precognitive type. No overall significance was found in either forward or backward displacement, however, and therefore the displacement scores will not be considered further in this report.[4]

Table 1

General Results

Series	Runs	Hits	Dev.	Av. Run Score	CR
I	400	2114	+114	5.28	2.85
II	400	2097	+ 97	5.24	2.42
Total	800	4211	+211	5.26	3.73
					P = .00019 3P = .00057

With the ESP scores significantly positive, the next step was to see how the overall deviation was distributed in relation to the mood ratings. It should be reemphasized at this point that the experiment was designed to explore this area, to see whether there were indications that certain kinds of moods would show some relationship to success in precognition tests. It was not designed to offer conclusive evidence on this point, nor can the exploratory analyses employed be said to do so. They are, however, of special interest in that they indicate in a general way the kinds of moods which accompanied much of the statistically significant overall deviation.

In determining the score distributions in relation to the mood ratings, the scores were first considered in terms of the three separate divisions of the scale. Thus each session score was tabulated first according to its physical rating, then according to its emotional rating, and finally according to its mental rating. There were general indications that in each of the divisions positive scores tended

[4] There was, however, a tendency for forward displacement to be associated with "Tired," physical rating 4 (140 runs; CR = 2.72), and with "Busy with everyday considerations," mental rating 4 (85 runs; CR = 2.44).

to be associated with ratings 1-3; this tendency was more noticeable in the physical and mental divisions than in the emotional. Items 4 and 5 in all three tended to be associated with chance scoring. There is a suggestion of an upswing of scoring level associated with items 6 and 7 in the physical and emotional scales (See Table 2).

Table 2

ESP Results Associated with the Seven Separate Ratings in Each of the Three Mood Scales

Rating	Physical			Emotional			Mental		
	Runs	Av. Run Score	CR	Runs	Av. Run Score	CR	Runs	Av. Run Score	CR
1.....	105	5.5	2.59	150	5.31	1.92	50	5.44	1.56
2.....	210	5.19	1.35	190	5.32	2.21	165	5.43	2.72
3.....	165	5.55	3.58	235	5.12	....	220	5.29	2.19
4.....	140	4.92	....	120	5.18	1.00	85	5.03	....
5.....	35	5.11	....	35	5.2	....	100	5.28	1.40
6.....	65	5.54	2.17	45	5.33	1.12	165	5.15	.97
7.....	80	4.97	....	25	6.24	3.10	15	4.96	....

In this analysis, of course, each division was considered as if it were independent of the others. On that basis the scale does not show any clear-cut relation between mood and ESP performance.

There remained the prime question whether the composite mood evaluation showed any relation of interest. But for such an appraisal it was necessary to find a simple common basis of comparison by which the units of ESP performance (the five-run session per subject) could be evaluated with respect to the multiple mood classifications. In order to do this, some means had to be found by which each rating within the combination could be classified in one of two ways rather than in one of seven. This might be called a common denominator of separation.

Since moods are ordinarily described as "pleasant" or "unpleasant" and since, because of this, each division of the scale was arranged in a rough gradation from pleasant to unpleasant, the pleasant-unpleasant difference seemed logically to constitute the necessary and basic contrast that was needed. Thus, if a cut-off point between pleasant and unpleasant ratings were assigned on each division of the scale, then each single rating could be classified as pleasant or unpleasant and four basic mood categories could be set

up: the "wholly pleasant" (three pleasant ratings), "predominantly pleasant" (two pleasant, one unpleasant rating), the "predominantly unpleasant" (two unpleasant, one pleasant rating) and the "wholly unpleasant" (three unpleasant ratings).

Dr. Gordon Mangan, who did not otherwise participate in the experiment, was asked to make such divisions for the three parts of the scale. His decision was that the pleasant ratings were these: items 1-3 in the physical scale, items 1-4 in the emotional scale; and items 1-4 in the mental scale; and he considered that all other ratings were unpleasant.

On the basis of Dr. Mangan's division of the ratings, I then separated the data into the four basic mood categories described above, so that sessions which were accompanied by pleasant ratings on all three parts of the scale were designated sessions carried out under "wholly pleasant" mood conditions, those which had received two pleasant ratings were designated "predominantly pleasant," and so on. This analysis showed that in Series I the wholly pleasant moods gave a deviation of +112, with a critical ratio of 3.91, while results in the other mood categories were insignificant. In Series II, the deviation in wholly pleasant moods was in the same direction, +50, but it was not significant, the probability value for the observed deviation in the expected direction being .047. On the other hand, while the wholly unpleasant moods in Series I were accompanied by chance results, those of Series II were associated with significant scores. (See Table 3.)

When the data of both series are considered together in terms of these separations, the wholly pleasant moods gave a deviation of +162, with a critical ratio of 3.91; the wholly unpleasant moods gave a deviation of +44, with a critical ratio of 2.54. The deviations in the other two categories were about at mean chance expectation. Thus it appears that the *consistently* pleasant or *consistently unpleasant* moods were accompanied by better results in precognition tests than a mixed condition of pleasant and unpleasant moods. These consistent combinations were associated with 63 per cent of the total runs, but with 98 per cent of the total deviation, while the mixed-rating moods accompanied 37 per cent of the total number of runs, but only 2 per cent of the total deviation. While

Table 3

DISTRIBUTION OF SCORES IN ACCORDANCE WITH NUMBER OF PLEASANT OR UNPLEASANT MOOD RATINGS

SERIES I

Subjects	Wholly Pleasant Ratings			Predominantly Pleasant Ratings			Predominantly Unpleasant Ratings			Wholly Unpleasant Ratings		
	Runs	Dev.	Av. Run Score	Runs	Dev.	Av. Run Score	Runs	Dev.	Av. Run Score	Runs	Dev.	Av. Run Score
R.V.d.C.	35	+27	5.77	..	..	....	10	−11	3.9	5	+ 2	5.40
F.G.	30	0	5.00	5	+ 4	5.80	10	− 3	4.7	5	0	5.00
S.C.	20	+ 3	5.15	15	+ 7	5.47	15	− 6	4.60	..	..	....
J.L.	25	+20	5.80	10	+ 4	5.40	15	−11	4.27	..	..	....
K.O.	25	+22	5.88	10	+ 4	5.40	15	+11	5.73	..	..	....
R.W.	40	+34	5.85	..	..	....	5	− 3	4.40	5	− 1	4.80
N.G.	25	0	5.00	10	+ 7	5.70	10	+ 5	5.50	5	− 4	4.20
W.N.	5	+ 6	6.20	15	− 2	4.87	20	+ 1	5.05	10	− 2	4.80
Total	205	+112	5.55	65	+24	5.37	100	−17	4.83	30	− 5	4.83

SERIES II

R.V.d.C.	23*	+ 8	5.35	..	..	....	12*	−10	4.17	15	+ 7	5.47
F.G.	35	+ 5	5.14	5	− 1	4.80	5	− 6	3.80	5	+ 6	6.20
S.C.	25	+10	5.40	15	−13	4.13	10	+ 4	5.40	..	..	....
J.L.	20	+ 5	5.25	5	− 2	4.60	20	+ 5	5.25	5	+ 6	6.20
K.O.	40	+14	5.35	..	..	....	10	0	5.00	..	..	....
R.W.	30	− 6	4.80	..	..	....	10	− 1	4.90	10	+13	6.30
N.G.	30	+19	5.63	5	+ 3	5.60	10	+ 2	5.20	5	0	5.0
W.N.	20	− 5	4.75	15	+ 8	5.53	10	+ 9	5.90	5	+17	8.40
Total	223	+50	5.22	45	− 5	4.89	87	+ 3	5.03	45	+49	6.09
Total Series I and II	428	+162	5.38	110	+19	5.17	187	−14	4.93	75	+44	5.58

*During one session, the subject was interrupted by a children's quarrel at the end of the third run. He rated the first three runs as wholly pleasant, but the last two as predominantly unpleasant.

this analysis is largely based upon *a posteriori* considerations of the data, the striking manner in which it separates the sessions into those with high and low ESP scores suggests that it may be useful for this purpose in further psychological studies of psi.

The more fundamental question of whether the level of ESP scoring and the mood combinations of the subject vary together requires a test of the consistency of the subject's ESP performance from session to session in relation to mood. Each subject's ESP score in each session was classified as being above or below his

overall average for the entire experiment, and it was further separated according to whether the subject chose *consistent* or *mixed* moods. This tabulation resulted in a fourfold table which was

Table 4

VARIATION OF ESP SCORES IN RELATION TO CONSISTENCY OF MOOD RATINGS

	Subject's ESP Session Score Above His Overall Average	Subject's ESP Session Score Below His Overall Average	Total
All Three Mood Ratings, Pleasant or Unpleasant	56	45	101
Mixed Ratings Showing Both Pleasant and Unpleasant Moods	23	37	60
	79	82	161

$\chi^2 = 4.41$ (1 d.f.)
$P = .036$

evaluated by the usual chi-square test. It gave a chi-square of 4.41 (1 d.f.), with $P = .036$. The detailed results of this analysis are given in Table 4. While this shows the results to be below the level of significance with regard to consistency, it does suggest that the overall separation of ESP scores discussed in the preceding paragraphs probably involves a degree of lawful relationship between ESP performance and mood ratings.

DISCUSSION

The evidence for precognition represented by the total deviation on the ESP scores has been strengthened to some degree by the analyses of the mood ratings, even though the specific analysis which gave the most interesting results was not expressly anticipated in the design of the experiment. That is, the relationship that was found between the mood ratings and the ESP results was one that had not been specifically foreseen—as so often happens in exploratory re-

search. The most meaningful suggestion that does emerge is clearly this: that whatever the three subdivisions of the scale were getting at, they were sufficiently different that the combined responses served as a measure of *something* seemingly relevant to ESP success. It appears that when a subject felt strongly enough about his mental state to mark all three divisions of the scale "pleasant" or "unpleasant," an intensity of motivation or readiness for vigorous reaction resulted which appeared to accompany the functioning of ESP. The three divisions of the scale are clearly somewhat interlocking or overlapping in their psychological implications. In a way, then, the subject who rated himself in the category of pleasant on all three was simply showing a quantitatively more extensively euphoric response. Psychosomatic unity being what it is, it is not unreasonable to think that the subdivisions of the scale may be looked at as a pattern which allows just such an overall quantification.

While this breakdown represented in Table 3 is interesting and important from the point of view of furnishing reassuring evidence of the functioning of precognition, it is to the measurement of mood-ESP consistency in Table 4 that we must turn for an indication of how far mental state actually relates to ESP success. This measurement, even though not conclusive, is definitely encouraging; and because of the importance of the basic question involved, it may merit special attention. Its particular significance stems from the fact that the mental states measured in this experiment (if "measured" is not too big a claim to make) are states that come and go and presumably may be induced. If, as has been emphasized often in recent years, one of the primary problems confronting parapsychologists is that of finding effective ways of eliciting and improving psi test performance, then even a finding that was only suggestive, such as this mood-ESP relationship, may take on value from this relevance. There is, however, some added warning in the fact that the comparison of the results from Series I and II did not show as much consistency as would probably be expected by those not familiar with the vagaries of the psi process.

One of the most important features of this experiment is one that might ordinarily be omitted from the report, because it is not a formal consideration; it was, in fact, an incidental aspect. This was the

spirit manifested by the subjects in response to the use of the rating scale. The subjects, especially during Series I, found themselves watching eagerly for definite identifiable moods in which to do a session of tests. Beyond doubt this gave much added interest to the project. There was advantage, too, in the fact that the precognition test procedure left so much freedom of choice and individual initiative to the subject himself. In each case it is presumable that the subject did the test because he really wanted, then and there, to do it. That alone might well be responsible for the significantly positive overall deviation.

It is noteworthy that for many subjects there seems to have been a personal challenge in the setup of the experiment and it may well have been that extreme mood was associated with stronger motivation in the test. It may be, also, that the use of such a scale is favorable in itself, since it probably tends to reduce any fear the subject may have of getting a poor score, disappointing the experimenter, and perhaps spoiling the experimental results. When the conditions associated with scoring variations are themselves the point of inquiry, even negative scores may have information value at least. The subject's response may be less inhibited under these circumstances.

These are speculative remarks. The value of the results may be largely in the suggestions they leave for thinking further about the conditions that affect the functioning of psi abilities.

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A RE-EXAMINATION OF DR. SOAL'S "CLAIRVOYANCE" DATA[1]

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Abstract: This is a report of a search for evidence of clairvoyance in the ESP results obtained by Dr. S. G. Soal and his associates (Mrs. K. M. Goldney and Mr. F. Bateman). The fact that other experimenters have obtained conclusive evidence of clairvoyance under well-controlled experimental conditions has raised the question as to whether Dr. Soal, in his GESP investigation, might not have obtained indications of clairvoyance that were overlooked. Mrs. Foster has examined the Soal results and has found only certain suggestive indications of clairvoyance in a limited section of the data. She has not encountered any significant body of evidence of that type of ESP. Special interest in the question is raised by Soal and Bateman's work with the Spanish girl, Maria, discovered by Mr. John Langdon-Davies. With Langdon-Davies Maria had a long record of good results that were as high for clairvoyance as for GESP. But in those carried out by Soal and Bateman, Maria gave only chance results with clairvoyance and significantly positive evidence with GESP. The question that arises is whether Dr. Soal and his associates may not introduce some psychological factor to the experimental situation that allows the operation of ESP (presumably telepathy) under GESP conditions but inhibits its functioning under conditions allowing only clairvoyance to occur.—Ed.

Introduction

The experimental evidence shows that subjects can succeed in ESP tests when they are asked to respond to physical objects or events (clairvoyance) and also that they can identify something that is known to another person (telepathy). If a particular investigator obtains statistically significant results under only one of these general conditions, that fact no longer has a crucial bearing upon the question of whether both clairvoyance and telepathy occur, but it may provide important clues to some of the psychological factors that affect ESP performance.

It is from the latter point of view that many research workers have long been interested in the fact that when Dr. S. G. Soal was experimenter he and his associates obtained highly significant results when their subjects worked under GESP conditions but do not

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seem to have obtained definite evidence of a nonchance factor when their subjects worked under clairvoyance conditions. Dr. Soal has done only the more general analyses of his clairvoyance data but has made his data available for more detailed study by other investigators. Some time ago I did some analyses searching for indirect evidence of ESP in the clairvoyance data of Dr. Soal's two earlier subjects, G.S. and B.S. When his more recent work with the Spanish girl, Maria, also showed highly significant results with GESP but did not give a significant total deviation on clairvoyance (**2**), I undertook a systematic study of the G.S. and B.S. clairvoyance data. In several years of work with each of these two subjects, Dr. Soal obtained consistently high positive rates of deviation under "telepathy" (GESP) conditions; on the other hand, under a condition which he describes as "clairvoyance," he did not obtain any result which seemed to him to be nonchance.[2]

Dr. Soal's conclusion that a nonchance ESP factor was demonstrated in his card-guessing experiments was fully supported by his telepathy results. The detail in which he analyzed his clairvoyance results was sufficient for the purpose of not claiming a nonchance factor in them. However, he goes a little further and says: "There seems, then, little doubt that, like Shackleton, Mrs. Stewart does not succeed in clairvoyance experiments, but only scores above chance expectation when the agent knows the order of the cards" (**5**, p. 211). Until the characteristics of ESP performance are more fully known, it is improper to assert a negative statement strongly. There is sometimes evidence, in an ESP situation, of such nonchance factors as position and/or decline effects

[2] For those readers who do not know just what conditions he refers to as "telepathy" and "clairvoyance," we give the following brief description: When a page of trials (two columns of 25 trials each, or 50 trials) was to be made, the agent, behind a screen, shuffled five key cards (each bearing a letter to indicate the name of an animal, such as Z for Zebra, E for Elephant, etc.) and laid them face down in a row from left to right. The card farthest to the left was position 1, the second was position 2, and so on to position 5 on the right.

If it was a telepathy test, the agent, after shuffling these key cards and putting them face down, turned them over to see what symbol appeared in each position and then put them face down again. For a test of clairvoyance, he did not look at the order of the target cards until the series of 50 trials was completed.

For both telepathy and clairvoyance, an experimenter, on the other side of the screen from the agent, used a prepared list of random digits to select cards numbered from 1 to 5 which he presented, one for each trial, at a window in the screen within the agent's view. The agent touched the corresponding key card in his row and tried to influence the subject by psi to make the correct response.

even when there is no overall deviation, and it therefore seemed to the writer that Dr. Soal's clairvoyance data should be analyzed for these effects as well as for others that might provide clues for further research.

The B.S. Data

It seems reasonable to assume that clairvoyance would function optimally if the subject was told that no one would serve as agent, rather than if he were left to presume that an agent was looking at the cards when actually he was not. In 32 runs done by B.S. (**6**, p. 50), he was told that the test would be clairvoyance. These gave an average of 5.75 per run of 25 trials on the direct (0) target. Dr. Soal scored his data also in terms of hits on the two just-preceding targets (−1 and −2 displacement) and on the two immediately following targets (+1 and +2 displacement). The average of 5.75 on 32 runs gives a CR of 2.12. Taken alone, this would be suggestive of ESP, but since it is selected from among these five possible scores, Dr. Soal properly ascribes it to chance.

A more detailed study of these runs, however, shows that there is some additional evidence of a nonchance factor. Dr. R. H. Thouless has pointed out that "of ten experiments done under these conditions, Shackleton on one occasion achieved the very considerable feat of making 58 correct guesses out of 200. This excess of 18 over mean chance expectation is one that by chance alone would not be equalled or exceeded more than once in about 700 times. When we take into account the fact that this was the result of the selected best of 10 experiments, it cannot be regarded as very convincing evidence that Shackleton could succeed under these conditions, but it remains sufficiently unlikely to be a chance result to make one unwilling to assert with any conviction that he could not succeed under 'clairvoyance' conditions" (**8**, p. 28).

When I undertook the more detailed study of the clairvoyance runs, it seemed to me that if nonchance factors not revealed by the total scores were present, they would be likely to resemble the nonchance effects in the alternating telepathy runs. One of the principal secondary effects discovered in the B.S. telepathy data was multiple determination, which means that B.S. scored higher on

forward and backward displacement when the forward and backward displacement targets were identical than when they were not the same (3). One would not normally expect to find this effect in data where the deviation was on the direct target, and I did not find it in the clairvoyance runs.

Next I analyzed the clairvoyance runs for secondary effects that have been found in other ESP data. The organization of ESP is sometimes such as to favor or disfavor the perception of card doubles, that is, of sequences of two or more identical target symbols (7), but this analysis of the B.S. clairvoyance data gave a chance result. When I analyzed for position of direct target hits in the column, however, I found that the whole of the target deviation occurred in the latter half of the column. The average score for the second half of the column is 6.58 (CR = 3.1; P = .0019). This average is not significantly less than the average of 7.02 obtained on the (+1) targets of the 38 alternating telepathy runs, nor than the average of 7.2 obtained on the second half of the telepathy runs. This second half-column average of 6.58 is contrasted with a first half-column average of 4.88. The critical ratio of the difference between these levels of scoring is 2.32 (P = .02). This suggestive difference is statistically independent of the general level of scoring and so may be taken as a further indication of a nonchance factor. The distribution of hits by the five segments in the run for the clairvoyance runs and the (+1) displacement in the telepathy runs are given in Table 1. The scores on the five segments of clairvoyance runs are not sufficiently different from one another to justify a conclusion that they are samples of different populations.

If these 32 runs which B.S. was told were to be clairvoyance were the only clairvoyance runs Dr. Soal had had his subject do, and if the decline analysis were the only criterion of success, we might consider a nonchance factor in these runs to be quite likely. Presumably the same level of scoring in the latter half of the clairvoyance and telepathy runs, respectively, shows the same amount of ESP. Why, then, do we have a chance level of scoring in the first half of the clairvoyance runs? That is, our clinical and theoretical problem is: Why do inclines and declines occur? However, other analyses on the same data did not produce any significant results.

Table 1

COMPARISON OF RUN SEGMENT AVERAGES FOR "KNOWN CLAIRVOYANCE" RUNS (DIRECT TARGET) AND ALTERNATING TELEPATHY RUNS (+1 TARGET) IN B.S. DATA

Segment	Clairvoyance Direct (0) Target 32 Runs	Telepathy Displacement (+1) Target 38 Runs
1	5.2	6.1
2	3.9	6.7
3	7.2	7.5
4	6.1	7.9
5	6.4	5.9

Likewise the same analyses were applied to the 20 clairvoyance runs done when B.S. was not told the agent was withdrawn from the experimental situation, and no significant results were found.

THE G.S. DATA

A larger number of clairvoyance runs was done by G.S. (**5**, p. 209-11). Most of them (96) were interspersed with the telepathy runs, although G.S. was not told that there had been any change in the usual telepathy conditions. In another eight clairvoyance runs, she was told of the changed conditions just before the run.

The average deviation of the G.S. clairvoyance runs is even less than that of the B.S. runs. There were 509 hits in the 104 runs, an average of 4.9 per run, which is a very close approximation to mean chance expectation. The first page of clairvoyance data gave seven hits in column A and 10 hits in Column B. The first half-session (4 runs) averaged 7.7 hits per run and would be significant if it could be considered as a unit. Although the second half-session contributed no deviation, the average run score of the complete first session was 6.83, which is suggestive at the 6 per cent level. Other sessions, however, contribute no significance in terms of total deviation, nor is there any general decline within the sessions.

The telepathy runs of G.S. exhibit many secondary effects and I have analyzed the clairvoyance runs for these effects and for others that appeared reasonable. The isolated high-scoring clairvoyance sessions by G.S. and B.S. suggested that there might be a positive effect in some runs and a negative effect in others. Since the

telepathy runs of G.S. showed position effects, I divided the trials on her clairvoyance pages into random halves to make a split-half reliability test. This came out positive, but not sufficiently so as to be even suggestive. (The A and B columns of B.S. also showed no correlation.) The combined results of the two subjects was also at chance.

The G.S. telepathy runs showed some decline of scoring within the column and a significant difference between the number of hits on odd *versus* even trials, especially for column B, the second column on the page (**4**). However, the 96 clairvoyance runs in which G.S. was not told of the change in conditions did not show any significant decline in either column even when the odd-numbered and even-numbered trials were considered separately (Table 2). A similar tabulation for runs selected as having scores of 6 or more also showed no significant differences. Position tabulations in the clairvoyance runs for forward (+1) and backward (−1) displacements also gave chance results.

Table 2

Comparison of Scoring in Two Halves of the Column Shown Separately for Odd- and Even-Numbered Trials in 96 Clairvoyance Runs of G.S.

	Column A Hits	Column B Hits
Odd-numbered Trials		
First half of column	65	58
Second half of column	58	69
Even-numbered Trials		
First half of column	52	49
Second half of column	55	50

In her telepathy runs, G.S. made significantly fewer hits on targets which were members of doubles, triples, etc. than on singles (**7**). No such effect was found in the clairvoyance runs on the (0) target or on (+1) and (−1) targets. Even the first session, which had a total deviation that bordered on significance, shows no difference in score on singles and doubles respectively.

Analysis of Other Possible Symbol-Position Codes in the Data of Both Subjects

The analyses above described were carried out on possible effects associated with the symbol-position code as given for each page. This code represented the order in which the agent placed the target cards, such as L in the first position, Z in the second, etc. However, since the agent in the clairvoyance runs did not know what the order of the symbols in the five positions actually was, he might have made some arbitrary association between position and symbol. Or the subject might have known by ESP which position the agent touched and have made some association between it and a certain symbol. For example, position one might have been associated with Zebra. Again, the agent might unconsciously have thought of the picture which had occurred in the same position for the preceding telepathy run; that is, he might have translated his pointings into the code most recently used. Effects discovered by this general approach might or might not afford evidence of clairvoyance.

A very general approach to the study of such an unconsciously introduced code in the clairvoyance runs is to ask whether there is any relation between the target for a given call and the target which occurs the next time the call symbol is repeated. For example, if the subject incorrectly calls Lion when the target is the symbol in the fourth position, will the target tend to be the symbol in the fourth position the next time the subject calls Lion?

When the clairvoyance runs were tabulated in this way, an unexpected negative deviation was found. There was a highly significant tendency for the target symbol on the subsequent trial to be *different* from what it was on the given trial. However, Dr. Pratt suggested that since this effect might arise as an artifact due to a small departure from randomness in the cards, I should repeat the analysis with control data provided by a cross-check. When the calls of each run were paired with the cards of another run, the same effect appeared to a highly significant degree and not significantly less than the effect shown by the clairvoyance runs themselves. It seems, then, that this analysis gives no definite answer to the possibility of coding. I turned then to an analysis of the several codes that seemed likely ones to me.

Analyses were made to detect possible codes which the agents alone might have introduced. I checked first on the assumption that each agent might have made an arbitrary association between position and symbol in the clairvoyance runs. No effect of this kind was found either individually for the two agents who served in the largest number of sessions or for the total of all agents.

Another possibility is that the agent might associate each position with the symbol that had occurred in that position in the preceding telepathy run. The total deviation obtained when the clairvoyance runs were scored in this manner was not significant, but in some respects the distribution of page scores did not seem to be chance. It might be that the code of the first page of a session would carry over to the second page, but that for later pages there would be a confusion among the more numerous possible codes. There were 26 clairvoyance runs done by B.S. and G.S. on the second page of a session following a telepathy page. These runs average 5.85 when checked by the code of the preceding page, a result that is suggestively greater than chance at the 3 per cent level. There are 142 other clairvoyance runs which are not done at the beginning of a session, and they average 4.895. The difference between these averages is .95, which is suggestive at the 3 per cent level.

This suggestion of a nonchance factor in the total deviation of clairvoyance pages coming second in a session after telepathy runs and scored by the code of the telepathy runs was pursued further for G.S.'s data into analysis of secondary effects known to be present in her telepathy runs to see if the same organizational factors could be found. All analyses gave chance results. There were only 14 runs available for study, however, and that sample size is very small for a study of secondary effects. A short series of 40 runs was done in which the letters C F H K T (indicating five animals) were used as targets. These gave a deviation of +24 (average score, 5.60; $P < .06$) when scored by the code of the preceding page. These selected runs do not show significant secondary effects. Neither do the pages selected as having a score of 14 or more hits on the targets of the preceding page (where the expected number of hits is 10). It is obvious, of course, that any nonchance effect found in the clairvoyance runs by these decoding analyses

would not be direct evidence of clairvoyance. Rather, it would indicate a persistence of ESP from the preceding runs in which the agent knew the code.

It seems to me that these attempts to "decode" the clairvoyance runs have not been definitely successful in revealing any concealed evidence of ESP. We cannot say with emphasis that there is no code; nor can we say that if conditions were favorable to ESP the subject would have been likely to introduce some code as a means of achieving success in his ESP task. When the agent for G.S. was first shown the target cards face-up, but then turned them over and was shown several other permutations of the five symbols before telepathy runs began, there was no significant deviation for the runs as scored by the targets based upon the original code. On four pages of trials for which the agent first looked at the official code and then was shown two other permutations, G.S. scored 37/200 on the official card-position code for each page, 38/200 on the first permutation presented, and 49/200 on the second permutation presented. Alternating runs done in the usual way "produced a score of 56/200 which is significant." Soal and Bateman conclude: "What seems clear is that the agent's original code is wiped from the slate of his subconscious mind, as it were, by the immediate impact of the succeeding codes" (**5**, p. 214). It may be that the clairvoyance conditions gave a similarly complex possibility of several codes. This would make the subject's choice of code a subtle and difficult question, especially for the clairvoyance pages done when he was not informed of any change in conditions.

Conclusion

It appears that there is a fairly strong suggestion of a nonchance factor in the 32 runs done by B.S. when he was told that the runs would be clairvoyance. The 20 runs done by B.S. when he was not told of the change in conditions and the 104 clairvoyance runs done by G.S. contribute little or no additional evidence. The data do not support any definite assertion as to the existence or nonexistence of a nonchance factor in the clairvoyance runs done by these two subjects. Also, the decoding analyses were largely negative. No conclusion is possible, therefore, as to whether these subjects could

or could not demonstrate clairvoyance when Dr. Soal was the experimenter.

Note on the Subject Maria

The recent publication of the Ciba Foundation Symposium on ESP (1) contains the first detailed report of the investigation of the ESP abilities of the Spanish girl, Maria, and from the details given, it is possible to get a clearer picture of the success of this subject in GESP and clairvoyance tests with different experimenters. Mr. Langdon-Davies, the main experimenter with this subject, reports that Maria was successful from the beginning of her ESP tests, both with and without an agent looking at the cards. In fact, the average in clairvoyance tests was slightly, though not significantly, higher than it was when telepathy was a possibility. Yet, as readers of the *Journal* may recall from the earlier, brief statement of the main facts of the case (2), when Dr. Soal and Mr. Bateman went to Spain to investigate the subject, Maria obtained only chance results in clairvoyance tests but did a significant series under telepathy conditions. The clairvoyance tests were done first, and they involved 1200 trials, carried out on the day of arrival and the following day, with a deviation of only +3. Then, after a day's interruption during which Mr. Bateman "made friends" with Maria, 2400 trials were made in which he looked at the cards. For these there was a significant deviation of +69. This rate of scoring was lower than Maria's best work; she had periods of sustained scoring in which her rate of hitting was more than twice the expected chance level. However, she had been scoring at about the same level in telepathy tests during the seven days preceding the arrival of Soal and Bateman from England as in the telepathy tests done by them.

The series of trials made during the Soal-Bateman visit was not arranged so as to allow a conclusion that Maria's failure to score in clairvoyance tests was due to the experimenter. However, the fact that she succeeded only under the conditions in which telepathy was a possibility, coupled with the well-known preferential success of Dr. Soal in tests in which a sender knows the targets, is enough to direct research attention once more to this difference between his work and that of most other ESP investigators.

The really important question is not whether Dr. Soal overlooked evidence for clairvoyance in the work of B.S. and G.S. Even if this study had led to a clear conclusion that he did so, the outstanding fact would still be that his subjects showed such striking differences in performance in the telepathy and clairvoyance tests. When all the facts are considered, the suggestion is clear that the experimenter was in some way unconsciously influencing the subjects so that they "failed" at clairvoyance. If such an experimenter-subject influence may operate in ESP tests, how is it exerted? To raise this question is not, of course, to imply any criticism of Dr. Soal or anyone else as experimenter. But now that increased understanding and control of psi are primary objectives of parapsychological research, this clue to a possible psychological factor in psi tests deserves the most serious attention of research workers.

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RESEARCH ON SPIRIT SURVIVAL RE-EXAMINED

By J. B. Rhine

The question of whether the personality, in any effective way or degree, endures beyond the death of the body is still of interest to most people, and by many it is regarded to be of the greatest possible importance. Whether science can answer the question, and how, if at all, it should be investigated are matters that are much more subject to disagreement. While there are many who have found the case for such survival sufficiently strong to warrant their own personal acceptance, the more careful and informed students of the problem generally agree that no really conclusive evidence of incorporeal personal agency has yet been reported.

It is, of course, the ideal of science to reach unambiguous answers to all such general questions of fact. The assumption is that, with appropriate methods of study, any question can in time be properly answered. Sometimes, as the history of the research on telepathy illustrates, a problem has to wait for certain other prior questions to be answered first. Also, there may be delay in adjusting existing concepts and methods to the issue at hand. But if an important question were abandoned while still obviously unanswered, as may be occurring in the case of discarnate personal survival, lack of evidence would not in any sense constitute a scientific solution. The mere fading-out of sufficient interest in the question to sustain actual investigation would not be justified as long as there remained anything further to be done to bring the issue into the focus of research.

Naturally an investigation in any science has to have data to work on. The studies made of the claims of spirit survival have thus far been based mainly on the effort to demonstrate spirit communication through mediumship. Perhaps the most convincing product of these inquiries has been the indication of appropriate purpose on the part of the hypothetical spirit personality, especially when the pattern has been revealed through fragments communicated through different mediums, some of them nonprofessional. There

have been impressive instances in which, for example, it appeared that a discarnate personality transmitted a peculiarly appropriate message to a designated living individual at a time of special crisis.

By the time the methods for the controlled study of mediumship were well developed, however, the heyday of popular belief in and investigation of spirit communication was over. It is quite impossible, of course, to say what value the earlier studies would have if they had been carried out under the more careful conditions such as are now routine; in other words, it is hard to say what they are worth as it is. Today, for example, in a proper mediumistic investigation, not only do the medium and sitter (or cooperator) not meet physically during the session, but in the annotation of records, the sitter will be asked to check, in addition to all the material communciated for himself, all of that intended for four or five others as well, without knowing which is his own. Under these conditions the sitter could not, without physical contact, give the medium any sensory cues or guiding influence; and likewise, with the objective methods of checking now in practice, he could not in any way give the results increased significance by an over-enthusiastic interpretation.

* * * *

It cannot be said, however, that it was these improved methods themselves that brought the research in mediumship to the comparative inactivity it has reached. The work of H. F. Saltmarsh with Mrs. Elliott and that of J. G. Pratt with Mrs. Garrett are enough to discourage that interpretation. There are a few studies in progress, too, even today, that would further support the point that the exercise of precautions in approaching a medium and in recording and analyzing the material communicated do not necessarily inhibit the flow of significant messages pertinent to the cooperator (or sitter).

Nevertheless, there has been not only a decline of popular interest in the problem of spirit agency and communication, but also an almost complete abandonment of the scholarly investigations made during the first quarter of the century. This is true not just in one country, but everywhere in the world. The popular shift might be attributed to loss of such persuasive influence as that il-

lustrated by the example of Sir Arthur Conan Doyle. But that still leaves the question of why such leadership itself has no succession. The reason for that is the falling-off of scientific interest in the problem.

This general decline of interest in survival seems to have been partly a consequence of the experimental psi research itself. The reasoning behind that judgment is fairly obvious and convincing. The best evidence of spirit communication through mediumship has always brought forth from critics the counterhypothesis that it was the result of telepathy. The supposition underlying this counterhypothesis was that the medium could have obtained by telepathy from living persons whatever knowledge not her own might be conveyed in her utterances. The spirit theory itself assumed a telepathic mode of communication from the discarnate agent to the medium. It supposed further, of course, the survival of an incorporeal personal entity, the spirit, which could exercise telepathic communication with the medium. Obviously, then, the countertheory of telepathy between medium and other living individuals such as the sitter himself was a simpler one by far than the spiritistic hypothesis and therefore preferable.

But so long as telepathy itself was a poorly established hypothesis, it did not provide a very strong counterargument to spirit communication. Also, in the material communicated by the medium there was a great deal of information that, strictly speaking, telepathy could not explain. It consisted of items which no *living* person knew. The hypothesis of clairvoyance might have explained these items, but clairvoyance was not taken seriously. The very uncertainty, however, over how much weight was to be attached to telepathy as a counterhypothesis to survival gave the investigation of it a new importance and led in the early thirties to the Duke experiments in ESP. In these researches not only was the question of telepathy reexamined and new methods developed for its investigation, but in addition, the experiments cleared up, as far as presently possible, the distinction between telepathy and clairvoyance. And as other investigations followed in other research centers, the evidence for both types of ESP was considerably increased. As the study was extended to cover questions of the independence of ESP from space

and time and was carried over to the related effect of psychokinesis, the established phenomena of psi came to include a range of functions that could in theory account for all types of mediumistic evidence. In short, if a medium had sufficient psi capacity, it was within the realm of possibility she could do anything through her own powers that had been considered proper to credit to the spirit communicators supposedly working through her.

Although no one had ever expressly formulated the fact, the very identical human capacities (of psi) that had been demonstrated in the laboratory had all been implicitly attributed to the communicating personality supposedly involved in mediumistic phenomena (just as, in all the religious systems, they had been generally credited to all divine beings). Now, therefore, that these powers had been verified as present, at least in some living beings, it became plainly more difficult to be sure of a discarnate source of any medium's messages. As the realization of this inescapable ambiguity gradually dawned on the seekers after proof of survival, they generally became less confident. There seemed to be no point left to the pursuit of the investigation of mediumship, especially over the now much more arduous and complex course which the better methods had come to represent—not, at least, until evidence could be offered that could not reasonably be attributed to the medium's own psi capacities.

But the psi discoveries do not provide the entire reason why parapsychologists, in the more recent period, have been diverted from research on survival. The continued advance of biology and psychology during the last half-century has in itself made the spirit hypothesis appear increasingly more improbable to the scholarly mind. The mechanistic (or physicalistic) view of man has become the mental habit of the student of science; and with the wide popular influence of science, the effect on educated men is well-nigh universal. The close parallel between thought and brain activity that suggests an inseparable union between mind and body has grown more complete as knowledge has spread. First, in the long story of evolution as it unfolds, the emergence of mind is portrayed as an accompaniment of developing cerebral structure. In the briefer span of the development of the embryo and the maturation of the individual, again mental life unfolds with the growth of

brain and glands. In the ups and downs of living, whether in health or disease, with shock or narcosis or sleep, or with anything else that affects the brain, the same faithful identification of mental function with body process appears with lawful regularity, as inescapable as death and taxes.

All this has been brought home more and more to the thinking public, especially with the rise of neurology, neuroanatomy, psychiatry, and clinical psychology. What part of a person *could* survive—and function—with no organic body to serve as its basis? Memory? But a narcotic, a shock, or a brain lesion will interfere with memory in a normal living person; and this indicates that memory depends on its organic foundation. Emotions? But they cling to memories, and come and go with them. Intelligence? In the living being this depends on a very delicate complex balance of physiological operations. How *could* it be "liberated" from this brain action without simply discontinuing? How, then, can one get a rational concept of survival as even a possibility?

With all this interweaving of mental and physical processes into the unified concept of the individual that science has developed, on the one hand, and on the other, the only impressive evidence of spirit survival itself devaluated so heavily as it has been since the claims based on mediumship have been re-examined and reappraised, it is no wonder that today even among workers in parapsychology there is little enthusiasm left for this purpose. Those who do retain a lively interest do so mainly through the persistence of an earlier standard of valuation of the evidence from mediumship.

As a matter of fact, the very act of re-examining the question of spirit survival today is, in the light of this review of the situation, one that cannot be undertaken without grave consideration. For one thing, a less favorable intellectual climate exists in which to pursue the inquiry; and such "climatic" circumstances can greatly affect the progress of a research as well as the destiny of those who pursue it. And there is also the fact that those who are still willing and able to lend financial support to the investigation of survival are fewer in number and more hesitant than their predecessors of forty years ago. At that earlier stage, large sums of money were literally pushed at reluctant universities for the investigation of spirit survival. It is

probable that all concerned, whether or not they realize it, have been to some extent similarly affected by a loss of confidence in what the outcome would be of a thoroughgoing and well-controlled research on the spirit hypothesis today. Sober hesitation can be observed on every hand and on every level among those who would actually bear the burden in a responsible attack on the problem.

* * * *

To the scientist who is sincerely devoted to the project, however, this change of attitude need not be a determining consideration. So long as the question has not been answered reliably, one way or another, it is still the same question to him. If it should have to give promise of being answered in the affirmative in order to justify investigation, the approach would be one-sided and the research might be rendered useless by such bias.

It is true, past investigations of survival have, all the way through, been cast in the form of frank efforts only to verify the hypothesis of spirit agency. This very approach itself implied that *failure* to find supporting evidence would be a comparatively meaningless outcome, that only positive results could be in any way either conclusive or significant. But time and further analysis have considerably changed the picture. First, no longer is the question brought to the fore primarily through the claims of a religious or occult group as it was in the nineteenth century. That wave of enthusiastic investigation has run its course and, as indicated above, has spent its force with an inconclusive result. The question now arises, rather, out of the very psi researches that a generation or more earlier owed their origin to the wave of spiritistic interest just referred to (and to the fact that psi was a counterhypothesis to spiritism). Out of these researches has now blossomed a concept of the nature of man that seems to approach the criterion of spirituality—as far as it can be defined. Capacities have been demonstrated that show none of the criteria of physicality that the somatic structure and organic processes of the individual manifest.

So we may say that even if there had never before been any claim of a spiritual category in nature, these results would make it necessary to invent one, or at least one that might at this point simply be called "extraphysical." On a minimal basis, then, it would

seem that the evidence of psi in its relation to space and time has already answered the question as to whether there is anything in human personality belonging to a spiritual order of reality. It is only necessary now to push on further and ask: How distinctive and independent is this extraphysical division of personality? Is it in any degree separable from the living organism? Can it in any way or form outlast the dissolution of its body? It will be only a matter of time and continued investigation until this nonphysical part of personality will come more definitely under the scrutiny of science. Then a whole range of questions associated with the concept of spirit will be properly studied by the most reliable methods scientific scholarship can devise.

As the matter now stands, the problem of independent spirit agency must be regarded in a framework of larger significance. It is nothing less than the problem of the natural destiny of human personality. Accordingly—and this is the essential point of the present argument—*every* true advance within this framework of centrally important knowledge will be of priceless value to mankind. We are not any longer concerned merely to confirm a faith or support a missionary cause. Rather, we are searching for the boundaries of that larger world of personal action which the psi discoveries have already dimly outlined as extending beyond the confines of a narrow physicalism. Thus we are prepared for any possible outcome of these further explorations and we are ready to profit from it. If, as we may expect, it brings a view of human destiny that is not wholly congruent to that portrayed by nineteenth-century spiritualism or by any other dated conception, we may also anticipate that it will be at the very least as helpful to the need for an understanding of the place of man in nature as man's knowledge of himself has ever proved to be in the past. In a word, such a program of investigation, properly recast and reoriented, could not lose in any way except by losing its way entirely.

* * * *

Parapsychology is least likely to lose its way if it always makes its approach via the actual occurrences which raise its problems and, as objectively as possible, tries to find the explanation of those occurrences. One such point of departure for the survival project lies

immediately before us in a re-examination of some of the original case material of this field. It will be recalled that the experimental work in parapsychology owes its origin largely to the occurrence of spontaneous psi experiences. The extent of the verification the experimental research has given to the psi hypotheses based on these experiences is now very considerable indeed. This verification has in recent years reached a stage that has made justifiable a return to the study of case material for still further hypothetical concepts to illuminate and sustain a broadened and diversified experimental program. This reconsideration is already yielding a harvest of value to the study of psi as a whole.

Within this general program of case work there is a specific project on the survival question. One of the fairly common types of psychic experience is that in which the impression is given of the agency of a deceased friend or loved one. In these experiences the appearance of the invasion of what would be called a spirit agent or communicator is, indeed, a frequent and sometimes a challenging feature. It is noteworthy, too, that these are experiences that owe no apparent connection to any given creed or philosophy. Whatever the influence of cultural factors may be, such experiences represent the individual's own interpretation of his experience.

Such a collection of personal data seems, at least on the surface, to furnish the raw material for a fresh study involving a new fact-finding approach to the question of discarnate personal agency. It confronts the parapsychologist with the problem of explaining the impression that a spiritistic influence produced the effect. If reports of this type can be obtained in sufficient number and can be subjected to adequate analysis and study, they may reveal aspects of human experience in some of its various transitions that will give clues needed for future research on survival. This is not to overlook the reservations and difficulties already mentioned, but rather to attempt to get beyond them.

Even at this preliminary stage of study, spontaneous cases suggesting survival have pointed to several possibilities that excite research interest. One block of these selected human experiences indicates a thread of personal motivation, of individual purpose, so peculiar to the one deceased individual identified that the kind of

message communicated could under the circumstances not be logically (or psychologically) credited to any other person living or dead. It will be interesting, and in any case profitable, to pursue this thread of peculiar purpose through the labyrinth of uncertainties as the inquiry advances. But there are also other special features of what men have considered manifestations of spirit agency that have never been fully examined for the key value they may have for opening the gates of new research. The question of why such remarkable effects cluster around the moment of death brings the research mind up sharply when confronted with the evidence en masse. These frequent death coincidences clearly warrant re-examination from the vantage point of today's somewhat broader knowledge and possibly more detached perspective.

There is no way of judging ahead of time how many different approaches will have to be devised in the pursuit of this now greatly broadened question of survival. Obviously success in bringing the matter to a focus on any one line of attack would obscure and discourage efforts in all other directions, just as the investigation of mediumship tended to do fifty years ago. If, however, frustration and uncertainty followed one effort after another, they would lead eventually to a complete loss of interest in the problem. Such an outcome would certainly follow if a point were reached when no further pocket of unexplained material remained which suggested survival. No one can properly say (as many have improperly maintained) that survival could never be disproved. Many a great issue has been solved by science in the past merely by the proper investigation and refutation of all the claims that helped to raise the question. When all these claims were leveled to the satisfaction of all, the question itself passed from the minds of men; so it was, for example, with the theory of spontaneous generation, and with many another theory of the past. So, too, it would be with the survival theory if none of its supports meet the tests of reliability.

Sooner or later, when some of the confusion that beclouds the issue is cleared away, it seems almost logically inevitable that the leadership dedicated to the investigation of the survival problem will turn to the idea of "letting the hypothesis establish itself" if it can. That idea is the old commonsense formula of making practice

vindicate principle, merely dressed up in scientific formulation. In other words, if there be a world of spirit entities or personalities of incorporeal nature, the most direct approach would be one in which the cooperative effort of such intelligent agencies would be solicited to contribute to the establishment of the hypothesis of their existence. Such a frontal approach, however, calls for well-developed preparation, if only to insure the high order of confidence and realism that would be involved. The more knowledge obtained as to what to expect of discarnate personalities, the better such a test of direct invitation and challenge could be made. But it will have to come eventually if the problem is pursued, unless it is solved in some less demanding way.

* * * *

In spite of the difficulties now recognized, there is really more cause for optimism of a kind in the present review than has been warranted for many decades. This is mainly owing to the fact that the problem of mediumship has become the broader question of the nature of personality with respect to the physical world. Having established that there is in personality something more than its physical substrate, parapsychology now has before it the further program of investigating this larger personal domain to see, among other things, what degree of independence there may be between that which is physical and that which is extraphysical in personality. These are questions of fact, and since successful beginnings in the answering of them have already been made, there should now be no turning back from the steps that logically follow. Rather, the important decisions now confronting us will involve only issues of expediency, efficiency, and wisdom of interpretation, and the strength of purpose to push the research program in the face of the indifferences and difficulties that our culture and heritage of the past have rolled up in the pathway of progress.

But some aspects of the events of the world today, too, are favorable. The dogmatisms of authoritarian belief regarding the destiny of the human spirit are crashing head on with the dogmatisms of mechanistic thinking deriving from the physical sciences. Both dogmatisms are bound up in the social and political institutions of modern life and have become instruments of power and power-seek-

ing. The resulting clashes and confusion will at least to a few thinking observers dramatize the importance of including the ultimate nature of man himself in a thoroughgoing scheme of investigation. Man's own peculiar characteristic, that which makes him a *person* as distinguished from a complex physical *thing,* will then become the central objective of factual study it has long deserved to be.

Whatever else might be said, the eventual outcome of such study, could hardly be disappointing to any, though it may well be surprising to all. If there be some who shrink from possible disillusionment, let them remember that the sciences have always *expanded* man's horizons on every frontier of knowledge. The later perspectives developed have *always* ranged beyond the wildest fancies of the founders in all the branches of science. This is the assurance of the past. But regardless of all other considerations, and whatever the verifiable realities of nature may be, there are, for the devotee of truth, no valid, enduring substitutes.

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NOTES AND COMMENTS

PARAPSYCHOLOGY CLOSES A CHAPTER

IN HIS ARTICLE "Some Present Impasses in Parapsychology,"[1] Dr. J. B. Rhine gave a realistic stocktaking of the situation in parapsychological research. The experimenters have come to an impasse: what next? Chapter I has closed; what about Chapter II?

It is possible that the opening of the new chapter may be quite different from the old. It should not be forgotten that the present apparent impasse is the result of success: the experimenters have accomplished what they set out to do. They have proved the existence of psi, that is to say, of telepathy and/or clairvoyance, beyond any reasonable doubt. Dr. Soal's book[2] shows that and may stand as a convincing record of the first completed stage.

Now the question arises, is there a worthwhile second stage? That is worrying some researchers. Suppose that we have demonstrated all that there is to demonstrate and that there is nothing much else for us to do? We seem to be faced with nothing but blind alleys and our completed but limited objective. Surely that merely means the end of a chapter? We must turn the page—by that I mean that we must consider our first inquiry proved and closed—and look for new ways to carry on new lines of attack.

As Dr. Rhine points out, the two major "checkmates"—I should prefer to say "checks," but perhaps Dr. Rhine does not play chess—are: (a) that the scientific world is still indifferent and unconvinced; and (b) that psi appears to be too elusive and intermittent to be effectively controlled either for experiment or use. From the point of view of Chapter I, these two checks are very depressing. From the viewpoint of a new stage to be planned and begun, they are valuable limitations. Instead of floundering helplessly in a void, we have been given at least two obstacles which may become objectives: to however small a degree, the way opens before us. If it is an unexpected way, beginning a new stage, that is exactly what we need when we have come to an impasse.

[1] *Journal of Parapsychology*, June, 1955.
[2] *Modern Experiments in Telepathy* by S. G. Soal and F. Bateman.

Let us examine our two obstacle-objectives:

a) First, in Dr. Rhine's words, is "the fact that the prevailing culture of general science is more vigorously opposed to parapsychology than could have been supposed. The physicalistic concept of man and life, and the whole habit of thought that goes with it, have become a faith to the student of science, and even the extraordinarily high standards of evidence set up in our field are not enough to counteract the blinding influence of this established belief."

Obviously the first objective is to modify the current theory, sufficiently at least to include the new knowledge. If psi exists, as we have proved that it does, any sound scientific theory must be able to include it. But this modification cannot be done merely by presenting theory-holders with indigestible facts, which none but the most unusually open-minded are likely to accept. We must present them with a theory, convincing in itself, which does not exclude the new knowledge. It is useless, for instance, to attempt to resurrect a discarded dualism—discarded for very good reasons; something more satisfactory must take its place.

In Chapter I, the experimenters (quite rightly) held the whole field; now it is time for thinkers to take a hand. Once it is realized what a delightfully complex and inconclusive field this offers, there should be no lack of philosophers to join in.

b) We do not find ourselves able to exercise any effective control over psi. This has been a serious handicap in experimenting. In spite of it, we have obtained overwhelming evidence for its occurrence, and for its being, under present conditions, elusive, fleeting, and not subject to conscious control. It is all to the good to have found out that much about it. We may in time find out some way of controlling it: then we shall know still more about it. Meanwhile let us consider what we do know.

The major trouble about this elusive, uncontrollable quality of psi is in experimenting—Dr. Rhine has some pertinent remarks to make about that—and in the fact that it makes psi useless. We cannot harness it to any practical or material purpose. Again, we should accept this and see where it directs us. After all, there is a deeper need than for material use. Mankind has been getting practical uses from science long enough, and very horrible and dangerous some-

of those uses are. The chief need for man today is not for cheaper and easier power, or some less clumsy means of communication, but some kind of answer to his spiritual and mental problems: not necessarily an optimistic answer, but an answer. The purely material conception of the scientists works well in science; it is not enough for human beings in their fulness. Can we do better, without reintroducing dualism? The onus is not wholly on the theorists: the researchers must give us materials to think about.

But research now will not be wholly experimental. Experiment is of course our most satisfactory weapon; wherever possible it must be used. But gathering and analyzing spontaneous cases gives us useful hints and information which can be got in no other way. G. N. M. Tyrrell's *Apparitions* and Dr. Louisa Rhine's work point the way.

For instance, we must know more about precognition if we are to find a satisfactory theory about it. Effect cannot precede cause in time as we at present understand it, and we shall never get the scientific world to accept precognition if we can only present an illogical explanation of it. What can the researchers—experimenters and analyzers—tell us about precognition? Can it be collective? Is it allied in any way with hypnotism? More often found in dreams than waking? In fact, if we are to develop a satisfactory theory about precognition (or any other psi phenomenon) there is no item of information about it which may not be suggestive.

When Chapter III of Parapsychology comes to be begun, it may include a big swing back to experimentation along the new lines suggested by thinkers and case-analyzers. We are at present only at the first beginnings of our subject.

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BOOK REVIEWS

THE UNKNOWN, IS IT NEARER? By E. J. Dingwall and John Langdon-Davies. London: Cassell and Co., 1956. 10/6 net. 174 pp. Index. (American Edition. New York: Signet Books, The New American Library of World Literature, Inc., 1956. 35 cents.)

The appearance in Great Britain and the United States of yet another popular survey shows that the public interest in parapsychological studies is increasing. In this book the emphasis is on the popular. The light, racy style, the dramatic case summaries without source references, and the snappy pronouncements about experimental results will not appeal to the more academic-minded. Nevertheless, much thought and background reading have gone into the preparation of this exposition. In spite of some carelessness in detail (in some of the statistical comments, for example) and in spite of a tendency inherent in their style of presentation to oversimplify the problems of evidence and of interpretation, the authors have produced a book with a useful function. It is short, readable, and entertaining and well calculated to present a sensible viewpoint to a section of the public that might otherwise never get to know about the results of serious parapsychological research.

DONALD J. WEST.

ERRATUM

The following substitution should be made for the last paragraph in "Interpretation of the Results of Forwald's Experiments in Placement PK" by Carroll B. Nash in the March, 1956, number of the Journal of Parapsychology, pp. 54 and 55:

Even if PK were a physical force, it could produce the lateral displacement of a die with considerably less expenditure of energy than Forwald calculated on the assumption that it pushes or pulls the die over the distance of its lateral displacement. The same lateral displacement would result from the inertia of the rolling die after a change in its direction, and the force necessary merely to change the direction of the die would be of a much lower order of magnitude than the force necessary to push or pull it over the distance of its lateral displacement. If PK impels the die the distance it is laterally displaced, as suggested by Forwald, the distance the die rolls in the longitudinal direction will not be affected by the distance it is displaced in the lateral direction. Dice with large lateral displacements will be found to have rolled as far longitudinally as dice with small lateral displacements. Since in Forwald's more recent experiments (**3**, **4**) the rolling plane was measured off lengthwise as well as crosswise in centimeters, the longitudinal distances the dice rolled may have been recorded. If that is so, data are already available for further testing his hypothesis.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests a single throw of the dice or other objects thrown.

HERE AND THERE IN THE LITERATURE

There is included in human nature an ingrained naturalism and materialism of mind which can only admit facts that are actually tangible. Of this sort of mind the entity called "science" is the idol. Fondness for the word "scientist" is one of the notes by which you may know its votaries; and its short way of killing any opinion that it disbelieves in is to call it "unscientific." It must be granted that there is no slight excuse for this. Science has made such glorious leaps in the last three hundred years, and extended our knowledge of nature so enormously both in general and in detail; men of science, moreover, have as a class displayed such admirable virtues, —that it is no wonder if the worshippers of science lose their head. In this very University, accordingly, I have heard more than one teacher say that all the fundamental conceptions of truth have already been found by science, and that the future has only the details of the picture to fill in. But the slightest reflection on the real conditions will suffice to show how barbaric such notions are. They show such a lack of scientific imagination, that it is hard to see how one who is actively advancing any part of science can make a mistake so crude. Think how many absolutely new scientific conceptions have arisen in our own generation, how many new problems have been formulated that were never thought of before, and then cast an eye upon the brevity of science's career.

. . . . . .

Is it credible that such a mushroom knowledge, such a growth overnight as this, *can* represent more than the minutest glimpse of what the universe will really prove to be when adequately understood? No! our science is a drop, our ignorance a sea. Whatever else be certain, this at least is certain,—that the world of our present natural knowledge *is* enveloped in a larger world of *some* sort of whose residual properties we at present can frame no positive idea.

William James, "Is Life Worth Living?"
from *The Will to Believe*